II.—*On Dew.* By Mr JOHN AITKEN.

(Read 21st December 1885.)

The immense amount that has been written on the subject of dew renders it extremely difficult for one to state anything regarding it which has not been previously expressed in some form. It has been examined over and over by minds of every type, and from every point of view; so that every possible explanation of the different phenomena seems to have been given, and so many passing thoughts recorded, that from the literary point of view the whole subject seems exhausted. As a necessary result, these different treatises are in many respects contradictory; and it would be quite impossible to construct anything like a consistent explanation and account of our subject, from the very voluminous writings of those who have treated it from the purely literary point of view, and whose ideas have been evolved from their inner consciousness, according to what seemed to them the fitness of things, and without questioning nature as to the truth of their conclusions. On the scientific side of the subject, however, the writings are not so voluminous, and additions to it are still required to enable us to determine which of the many conflicting opinions are correct.

In ancient times it was thought that the moon and stars had an important influence on dew, probably because there is most dew on those nights when these orbs shine brightly on the earth; thus confusing two things which have a common cause, and making one the effect of the other. ARISTOTLE placed the knowledge of this subject far in advance of his time. He defines dew to be humidity detached in minute particles from the clear chill atmosphere. The Romans, led by the writings of PLINY, returned again to the primitive idea that dew fell from the heavens. This idea retained its position during the course of the Middle Ages. Then began an endless variety of theories, such as, that the air is condensed into water by the cold, that the moon's rays caused it, and so on.

In the beginning of the eighteenth century clearer ideas began to be formed, and a reformation took place, in which, as in most reformations, the swing of the pendulum went to the extreme on the opposite side. Dew was no longer believed to descend from the heavens, for GERSTEN advanced the idea that it rose from the earth; and in this opinion he was followed by M. DU FAY and Professor MUSSCHENBROEK, the latter, however, afterwards made some observations which caused him to change his opinion. GERSTEN was led to think that dew rose from the ground, because he often found grass and low shrubs moist

with it, while trees were dry. M. Du Fay followed up these observations with experiments, made by placing sheets of glass at different heights from the ground. He found that dew formed on the lowest pane first, and only appeared on the highest at a later hour; he also found that the lowest pane collected most moisture. Other observers gave somewhat different explanations of the phenomena connected with dew; but owing to a want of clearness, the subject did not advance much till the masterly *Essay on Dew* by Dr Wells made its appearance.

Dr Wells' experiments were so simple, and his interpretation of the different phenomena connected with dew so clear, that he has been justly considered the great master of this subject. In his Essay he struck a medium between the two previous theories as to the source of the moisture that forms dew. He did not think with the ancients that it fell from heaven, nor with Gersten that it rose from the earth, but that it was simply condensed out of the air in contact with the surfaces of bodies cooled by radiation below the dew-point of the air at the place. This opinion has, so far as I am aware, been generally received up to the present time.

Some experiments I have recently made on this subject have caused me to differ entirely from Dr Wells as to the source of the vapour that forms dew. As everything written by Dr Wells is, so to speak, stereotyped and final, there seems to be the greater reason that any of his conclusions that seem doubtful should be carefully criticised and fully investigated; I shall therefore give an account of the experiments that have caused me to differ from so great an authority.

Dr Wells thought that almost all the moisture deposited as dew at night was taken up by the air during the heat of the day; so that, according to his idea, vapour ascended from the earth during the day, and again descended and became condensed as dew on the surface of the earth at night. My observations have led me to the very opposite conclusion. All my experiments indicate that dew, on bodies near the surface of the earth, is almost entirely formed from the vapour rising at the time from the ground; at least this would appear to be the case generally in this climate, to which my experiments have been confined.

After Gersten gave his reasons for supposing that dew rose from the ground, and Du Fay extended the subject, Dr Wells combated their conclusions, and successfully showed that their experiments did not prove that vapour rose from the ground, and that all the phenomena adduced in favour of their theory could be equally well explained according to his own. With regard to Du. Fay's reason for thinking that dew rises from the ground—namely, that it appears on bodies near the earth earlier than on those at a greater height—he says:* "But this fact readily admits of an explanation on other grounds, that have already been mentioned. 1. The lower air, on a

**An Essay on Dew*, by William Charles Wells, p. 109.

clear and calm evening, is colder than the upper, and will, therefore, be sooner in a condition to deposit a part of its moisture. 2. It is less liable to agitation than the upper. 3. It contains more moisture than the upper, from receiving the last which has risen from the earth, in addition to what it had previously possessed in common with other parts of the atmosphere." Then he goes on to give reasons why vapour cannot be rising out of the ground, but adds, that some of it must be from this source, as bodies near the surface of the ground get dewed sooner than those higher up, though equally cold with them, but says, "the quantity from this cause can never be great," and proceeds to give his reasons, which are not altogether satisfactory, and need not be quoted here. He then sums up as follows :—"These considerations . . . . . warrant me to conclude that on nights favourable to the production of dew, only a very small part of what occurs is owing to vapour rising from the earth; though I am acquainted with no means of determining the proportion of this part to the whole."

I shall now proceed to detail the observations which have caused me to differ from the conclusion so distinctly set forth by Dr WELLS in the above quotations. I need not say that all my experiments only confirm the conclusions of that observer as to the formation of dew—that is, as to the conditions most favourable for the deposition of moisture on the surfaces of bodies during dewy nights, while the earth is radiating heat into space. The point on which we differ is as to the source of the vapour that condenses on the radiating surfaces—a point which Dr WELLS admits there were no facts to determine, his own opinion being formed by experiments that did not bear directly on the subject.

When I began to doubt the truth of the generally received opinion as to the source of the vapour, I found a difficulty in beginning my investigation, as it was not easy to arrange experiments to give a direct answer to the question. My intention at first was to test, by means of a delicate hygrometer, the humidity of the air at different heights from the ground and under different conditions. This plan had, however, soon to be abandoned, owing to the impossibility of making anything like accurate observations with any instruments at present in use.

For some time I have had in my possession a hair hygrometer constructed by CHEVALLIER of Paris. This form of instrument is perhaps one of the best for the purpose; yet on making a few test experiments with it, for the special purpose under consideration, its indications were found to be nearly valueless. For instance, if the instrument was removed from saturated to drier air, and again replaced in the saturated, it was impossible to get the pointer back again to the same position on the scale; and as the amount of dryness it would be required to measure was a very small degree removed from saturation, the error in the indications might be greater than the actual amount of dryness.

Then again, all such hygrometers, as well as wet and dry bulb thermometers, will have their indications affected by radiation; they will surround themselves with an envelope of cooled air, as there is but little wind during the time the observations require to be made. Their indications would therefore be of little value, and investigation by means of them had to be abandoned.

What first caused me to doubt the present theory, and led me to suppose that dew is formed from vapour rising from the ground, was the result of some observations made in summer on the temperature of the soil at a small depth under the surface, and of the air over it, after sunset and at night. On all occasions in which these temperatures were taken, the ground a little below the surface was found to be warmer than the air over it. It is evident that, so long as these conditions exist, and provided the supply of heat is sufficient to keep the surface of the ground above the dew-point, there will be a tendency for vapour to rise and pass from the ground into the air, the moist air so formed will mingle with the air above it, and its moisture will be condensed, forming dew wherever it comes in contact with a surface cooled below its dew-point.

These considerations suggested another method of experimenting than by the use of hygrometers. If vapour is really rising from the ground during night, it seemed possible that it might be trapped on its passage to the air, and that this might be accomplished by placing over the soil something that would check the passage of the vapour, while it allowed the heat to escape. To carry out this idea, I placed over the soil shallow boxes or trays, made of tinplate and painted. These trays were 3 inches (76 mm.) deep, and more than a foot (305 mm.) square in area; they were placed in an inverted position over the soil to be tested.

The action of these trays will be somewhat as follows:—Supposing the roof of the small enclosure formed by the covering tray is not by the passing air or by radiation cooled below the temperature of the ground. Then evaporation will cease when the air between the tray and the ground is saturated, and no dew will collect on the inside of the enclosure. But if the tray is cooled below the temperature of the ground, vapour will condense on the inside, and more vapour will rise from the ground to supply its place, and this will go on so long as the ground is the warmer of the two. The effect of these trays will be very much the same as if there was no enclosure, and the air over the grass was nearly saturated, motionless, and of a lower temperature than the soil. But it is evident the trays will check the evaporation on most nights, on account of the slow circulation inside, and also on account of the air inside being always nearly saturated, which is not the case outside the enclosure, so that under most conditions it seems likely there will be less evaporation under the trays than outside them. This will be particularly the

case on those nights when there is wind, and the air is not saturated, a condition which seems to be very frequent in our climate at ordinary elevations. We must remember that the air may not be saturated when dew is forming; and the dew-collecting surface requires to be cooled below the temperature of the air before it collects moisture.

In experimenting with these trays different kinds of ground were selected, and the trays placed over them after sunset, that is, after the earth had ceased to receive heat, and the heat-tide had begun to ebb. They were generally examined between 10 and 11 P.M., and again in the morning.

Dew on Grass.

Confining our attention to the trays placed over grass, the result of the experiments was that, on all occasions yet observed, there was—1. Always more moisture on the grass inside the trays than outside. 2. There was always a deposit of dew *inside* the trays. 3. There was often a deposit outside the trays, but the deposit on the outside was always less than on the inside, and sometimes there was no deposit outside when there was one inside.

Now I think these facts prove that far more vapour rises out of the ground during the night than condenses as dew on the grass. This excess is evidenced by the greater amount of moisture on the grass inside the trays than outside, and by the amount of dew condensed inside the box. Under the ordinary conditions found in nature, this excess is carried away by the wind and mixed up with the air, while some of it is deposited on bodies further away from the ground. It should be noticed that the inside of the tray was more heavily dewed than the outside. This shows there was a higher vapour tension inside than outside the enclosure, which proved that the vapour rising from the ground outside the tray had got mixed up with drier air, as it did not form so heavy a coating of dew as the inside air, even though it had the advantage of a slightly lower temperature than the inside, on account of it being the side of the metal from which the heat was radiating.

It may be as well to notice here some objections that may be made to this way of testing the point. It may be said, that though so much vapour does rise under these trays, yet if they were removed and the grass freely exposed, the vapour would not rise, and that the vapour rises because the tray keeps the ground under it warm. Observation certainly shows that the ground under the trays is kept slightly warmer than outside them. At night a thermometer is higher on the grass under the tray than on that outside, and next morning the ground at 3 inches below the surface is from 1 to 2 degrees warmer under the tray than outside its influence. This objection to the protecting influence of the trays has an appearance of reason about it; but if we examine the facts, I think it will be admitted that instead of being an argument

against this method of experimenting it is rather a reason for it. We must remember the tray does not heat the ground; it does not add anything to its store of heat, and enable it to evaporate more moisture; it simply prevents so much of its store of heat escaping. Now heat escapes from the ground at night in two ways—first, by radiation, and second, by absorption—to supply the latent heat of evaporation. From the area covered by these trays radiation goes on much as at other places; the painted metal will radiate as much heat as the grass, but evaporation is checked, as there is but little circulation under the trays; and further, there is the heat recovered by the condensation inside the box. It would thus appear that the reduced evaporation and heat of condensation will be the principal causes of the higher temperature inside than outside; so that the trays, instead of increasing the evaporation, would rather seem to decrease it; and that the lower temperature outside is due to the greater evaporation there taking place, as both surfaces are exposed to the same loss by radiation.

There is an objection that might be made to the whole theory that dew is formed from vapour rising from the ground. It might be urged that it is impossible for the vapour to rise from the ground, and that these trays interfere with the conditions existing in nature. On a cold clear night, for instance, when the grass gets cooled before the dew-point, it might be said that it is quite impossible for the vapour to rise up through it, as it would be all trapped on its passage to the surface by contact with the cold blades, and that the trays placed over the grass prevent this condensation by stopping the radiation from the grass, and thus they allow the vapour to come up.

A little explanation will, however, show this objection to be groundless. On a dewy night no doubt the top of the grass is at a temperature below the dew point, and if we may take the temperature of a thermometer placed on the grass to be the same as that of the grass, which we may do without sensible error, if we then remove the thermometer and place it among the stems of the grass, the thermometer will rise; and if we place the bulb among the stems close to, but not in the ground, we shall find it to be very much warmer than at the surface. On dewy nights I have frequently found it as much as 10 to 12 degrees warmer. From this we see that the warm air diffusing upwards with its burden of vapour only meets with a very small amount of surface cooled below the dew-point, so that the greater part of the vapour is free to escape into the air.

Fairly considered, I think these trays more nearly represent natural conditions than might at first sight appear. Indeed, precisely similar results have been observed with natural conditions. If we examine plants with large blades, we shall often find, on dewy nights, that those leaves which are close to the ground have their under surfaces heavily dewed, while their upper surfaces

are dry. The effect of the trays is very much the same as that of these large leaves on a perfectly calm night. The only difference is, the trays will lose more heat on account of their better conducting power, and more vapour will be condensed under them than under the bad conductor, while the temperature of the soil will be more nearly reduced to what it would have been if no large close surface prevented the free evaporation.

The experiments described were made in August and September, when the ground was very dry, owing to the unusually small rainfall during the previous months. On all occasions the inside of the tray was dewed, however dry the soil, and the inside was always more moist than the outside.

After these experiments were made, another method of testing the point under investigation suggested itself, and though, unfortunately, rather far on in the season for satisfactory work of this kind, I at once proceeded to carry it out, as it afforded a means of checking my previous experiments with the trays; but by this time October had arrived, and the conditions had very much changed. The temperature had fallen considerably, and the rainfall had greatly increased the humidity of the soil.

It is very evident that if vapour continues to rise from the ground during dewy nights, as well as during the day, the ground giving off vapour must lose weight. If this could be shown to be the case, it would prove in a more satisfactory manner than the previous experiments that vapour does rise from the ground during night, and that, therefore, dew on bodies near the surface of the ground is really formed from the vapour rising at the time, and not from the vapour that rose during the day.

In the first week of October experiments were begun to test this point, by weighing a small area of the surface of the ground, before and after dew had formed, to see whether the ground continued to give off vapour or not while dew was forming. For this purpose a number of shallow pans 6 inches (152 mm.) square and $\frac{1}{4}$ inch (6·3 mm.) deep were prepared. One of these pans was selected, and a piece of turf slightly smaller was cut from the lawn and placed in it. The pan with its turf was then carefully weighed with a balance sensitive enough to turn with $\frac{1}{4}$ grain; but in experiments of this kind, which must be done quickly, accuracy of only one grain was aimed at, lest the time required for more accurate weighing might cause loss of weight by evaporation. To prevent loss from this cause, the weighing was done in an open shed.

The turf was cut at sundown, and when dew began to form. The earth was removed from it till it weighed exactly 3500 grains (226·79 grammes). The pan with its turf was then rapidly restored to the lawn, and put in its place, where the turf had been cut out, and in as good contact with the ground as possible. The pan and turf were then brought back, the under side of the pan carefully cleaned and dried, and all weighed again to make sure nothing was

lost in the manipulations; after which it was again restored to its place in the lawn, and left exposed while dew was forming. A few experiments were made in this way, in all of which the ground was found to lose weight. For instance, on the 7th October, the small turf freely exposed to the sky at 5.15 P.M., when weighed again at 6.30 P.M., was found to have lost $5\frac{1}{2}$ grains (0·356 grammes), and by 10.15 P.M. it had lost 24 grains (1·555 grammes). Fuller particulars of these experiments will be given further on.

In making these experiments, the first thing done was to sink two thermometers in the ground, one to a depth of 3 inches (76 mm.), the other to a depth of 1 foot (305 mm.), and to place a third thermometer on the surface of the grass. Readings were taken when the experiment began, and again when the pans were removed for weighing. During the time the turfs were exposed, generally about 5 hours, the soil at 3 inches below the surface lost from 2 to 5 degrees, while at 12 inches the loss was small. No doubt part of the heat was lost by radiation, but in grass-land, where the surface of the soil is protected by a fairly good non-conductor, much of the heat will be spent in evaporating the moisture.

These experiments prove clearly that under the conditions then existing, the soil loses weight, and that vapour really rises from the ground even while dew is forming; therefore the dew then found on the grass must have been formed out of the vapour rising from the ground at the time. The dew on the grass was, in fact, so much of the rising vapour trapped by the cold grass. The blades of grass acted as a kind of condenser, and held back some of the vapour which would have escaped into the air.

It must not be supposed that these experiments in any way contradict the well-known observations of WELLS and others who have worked at the subject. It has long been the custom to expose different substances to radiation during the night, and to estimate the amount of dew on different nights by the *increase of weight* due to the moisture collected on them. It must be noticed that the conditions of the two sets of experiments are quite different. In those for estimating or measuring the amount of dew, the collecting body must not be in heat communication with the earth, an essential condition being that it shall receive no heat by conduction from surrounding bodies; whereas, in the experiments with the turf, the essential condition is that the body experimented on shall be in as good contact with the ground as possible. The result of these two conditions is, that in the former, the exposed surface loses heat by radiation into space, and soon gets cooled below the temperature of the air, and when cooled below the dew-point, dew collects upon it; while in the latter case the exposed surface is in good heat communication with the ground, and tends to keep hotter than the other surface; then being always moist it tends to give off vapour, which diffuses away from the hot ground and escapes into the air above, but in part is trapped by coming into contact with the cold grass.

The experiments were generally stopped at night. It would be of no use to let them go on till morning, unless one were in attendance at sunrise; for the early morning heat radiated from the sun and sky would cause an increased evaporation, and make the loss appear too great. On one occasion, however, when the morning was dull, weighings were made, and the soil was then found to have lost weight during the late night and morning.

The following simple observation is sufficient to convince us that, under the ordinary conditions of our climate, vapour is almost constantly escaping night and day from soil under grass. Go out any night, but it is best when terrestrial radiation is strong, place one thermometer on the grass, and push another under its surface, among the stems, but it need not be into the soil, and note the differance in temperature. As an example, I found, at 10.45 P.M. on the 10th October, this difference to be as much as 18·5 degrees. The thermometer on the surface of the grass was 24°, while the other, only about $1\frac{1}{2}$ inches underneath it, and not in the soil, was as high as 42°·5, the temperature of the air at the time being 32°·5. Of course, this difference varies, and is not always so great as on this occasion, when the sky was clear and the air still. An experiment of this kind causes us to doubt the value of the radiation observations made by comparing the readings of a thermometer placed on the grass with the temperature of the air in the screen; because the temperature of the thermometer on the grass varies greatly according to its position. If its bulb is supported near the tips of the stems, the temperature is much lower than when it is allowed to press the grass close to the ground, because in the latter position it receives a good deal of heat from the earth.

It might be objected that these experiments having been made late in the year, and when the soil was damp, they do not prove that evaporation would take place in summer when the soil was dry. Other considerations, however, lead us to suppose that this nightly evaporation does go on even after a continuance of dry weather, though I have no direct experiments to prove it, other than those made with the inverted trays. But I find that soil, after it has been kept for some time in a house, and when it looks dry and incapable of supporting vegetation, still gives off vapour, and saturates the air over it. This was shown by placing over some dry-looking soil a glass receiver, in which was hung the hair hygrometer. The instrument soon showed an incease of humidity inside the receiver, and after a time indicated saturation. To check the reading of the hygrometer, it was quickly removed and placed in saturated air, when it was not found to change its reading.

Now as soil, even when it appears dry, tends to give off vapour, and saturate the air in contact with it, it is evident that under most conditions of our climate the vapour tension at the surface of the ground, amongst the stems of the grass, must, owing to the higher temperature, be very much greater than

at the tops of the blades; and as the air and vapour are warmer, they tend to rise and diffuse themselves, and so come into contact with the colder blades at the surface, where the moisture gets deposited as dew.

Having proved that, under the conditions existing during the experiments, the ground was giving off vapour during the night, I then proceeded to test the value of the observations previously described, and which were made by placing shallow trays over the grass, in order to see if those experiments were of any value. A small tray, similar to those used in the earlier experiments, was prepared. It was made to fit tightly into one of the shallow pans, in which, as before, was placed a small turf cut from the lawn. After the turf and its pan was weighed, the tray was placed over it, and the whole removed, and put in its place in the lawn. This was done at the same time as the other experiment previously described, in which the turf and pan, after being weighed, was freely exposed to radiation and evaporation. The result was that the tray was found to check the evaporation. The inside of the covering tray was dewed very much like another one placed over undisturbed grass. The turf covered by the tray lost only 6 grains (0·388 grammes) during the five hours, or about $\frac{1}{4}$ of the amount lost by the one freely exposed to the air. This shows that the trays check the evaporation; we may therefore conclude that the amount collected by them is less than would be given off by the exposed parts of the grass.

There seems to be reason for supposing that the amount lost per unit of area in these experiments, with the freely exposed turfs, is too low an estimate for the loss of the lawn at the parts where it was undisturbed, because the under sides of the pans were not in good contact with the ground beneath them. The experimental turf would not therefore be so warm as the rest of the ground, and its evaporation would therefore be less. Most of the heat was conveyed upwards towards the experimental turfs by the rising vapour, which condensed on the under sides of the pans on which the turfs rested, as they were always found to be dripping wet underneath when removed from the soil.

The question now comes to be, Does this evaporation take place from grass-land on all nights and in all weathers? So far as my observations at present go, evaporation is constantly going on, however strong the radiation. On all nights on which the inverted trays have been exposed, dew has collected on their inner surfaces. There is, however, an indirect way of testing this point which may be noticed here, as it is specially applicable to observations on grass land. As soil capable of supporting vegetation tends to saturate the air in contact with it, it will be admitted that so long as the soil is hotter than the air in contact with the grass, vapour will tend to diffuse upwards. Now I find by placing a minimum registering thermometer on the grass, and another on the top of the soil among the stems of the grass, that there is always a difference

between the minimum *on* and *under* the grass, often amounting to a considerable number of degrees, this difference being greatest on nights when radiation is strongest, and least when windy and cloudy. It is only as the day advances that the temperature *on* the grass approaches that *under* it; this is caused by the upper thermometer being heated by solar radiation sooner than the lower; but as the air is by this time drier, there is no tendency for it to lose moisture by contact with the colder soil, though some of the dew condensed on the grass will, after it evaporates, diffuse downwards, and condense on the soil. It may therefore be safely concluded that, on almost all nights in this climate, vapour does rise from grass-covered land, and it is this vapour that we see as dew on the exposed surfaces of the grass.

Dew on Soil.

While the experiments previously described were being made on ground covered with grass, parallel ones were made on bare soil. The inverted trays placed over soil always showed a greater amount of condensed vapour inside them than those over grass. Sometimes there was a heavy deposit of dew inside, while there was none outside. This would be owing to the soil radiating directly to the trays, and to the amount of heat brought up and conveyed to the trays by the vapour. The temperature of the trays was thus in some cases kept above the dew-point of the air outside.

Experiments were also made by weighing a small area of the surface soil, to see if it also lost weight like the grass-land during dewy nights. One of the small pans was covered with a thin layer taken from the top of the soil. The pan and its soil was then weighed and put on the surface of the bare ground at the place where the soil had been taken out. It was left exposed the same time as the other trays with the turfs. On weighing, the soil was found to have lost 23 grains (1·490 grammes) in five hours, or nearly the same as the turf. Alongside this pan was placed another one of the same area, and with the same weight of soil, but covered with a small tray, to see whether the covering trays decreased the evaporation from soil as well as from grass. The result was the same as was found with the turf—a decrease in the evaporation. The protected soil lost only 8 grains (0·518 grammes).

The following are the details of a few of the experiments on grass-land and on bare soil made on different evenings, and show the temperatures and the loss of moisture per 0·25 square foot, or 0·023225 square metre, during the experiments:*—

* Throughout this investigation I have adhered to the Fahrenheit scale, as it is the one generally used for meteorological purposes in this country, and because it possesses what appears to me practical advantages over the Centigrade scale. The degrees are of a more suitable size, and combine ease in reading with accuracy. This scale also avoids a fruitful source of error, experienced by many, when taking readings above and below zero.

October 7, 1885. 5.30 p.m.

			Grass.	Soil.
Temperature of soil,	3 inches below surface,		47°·5	46°
" "	12 " "		47°·5	44°

At 6.30 p.m.

Grass exposed,	lost	$5\frac{1}{2}$ grains,	or	0·356 grammes.
" under tray,	"	3 "		0·194 "
Bare soil exposed,	"	$5\frac{1}{2}$ "		0·356 "
" under tray,	"	$2\frac{3}{4}$ "		0·178 "

At 10.30 p.m.

		Grass.	Soil.
Temperature on surface,		36°·5	40°
" of soil,	3 inches below surface,	44°·5	42°·1
" "	12 " "	47°	44°

Grass exposed,	lost	24 grains,	or	1·555 grammes.
" under tray,	"	6 "		0·388 "
Soil exposed,	"	23 "		1·490 "
" under tray,	"	8 "		0·518 "

October 12, 1885. 5.30 p.m.

		Grass.	Soil.
Temperature of soil,	3 inches below surface,	44°	45°
" "	12 " "	44°·5	43·5

At 10.15 p.m.

		Grass.	Soil.
Temperature on surface,		31°·5	35°·5
" of soil,	3 inches below surface,	42°	40°·2
" "	12 " "	44°	43°·5

Grass exposed,	lost	30 grains,	or	1·944 grammes.
" under tray	"	6 "		0·388
Soil exposed,	"	22 "		1·425
" under tray,	"	$6\frac{1}{2}$ "		0·421

There was a little wind on this occasion, and very little dew formed. During the night the min. *on* the grass was 28°·5, under it 41°; and it was not till 10 o'clock next morning that the thermometer *on* the grass was as high as the one *under* it.

The following reading were taken about the 20th October, the exact date unfortunately is omitted in note-book :—

5.15 p.m.

Temperature of air—Dry bulb, 42·5 ; Wet bulb, 40.

		Grass.	Soil.
Temperature on surface,		39°	42°
" of soil,	3 inches below surface,	46°	48°
" "	13 " "	46°·3	46°·1

At 10.40 P.M.

Temperature of air,	Dry bulb, 38°	Wet bulb, 36°	at 4′ 0″	
" "	" $33\frac{1}{2}$	" 33°	near ground.	

		Grass.	Soil.
Temperature on surface,		31°	34°
" of soil,	3 inches below surface,	44°·2	43°
" "	12 " "	46°	46°

Grass exposed,	lost	9 grains, or	0·583 grammes.
" under tray,	"	8 "	0·518 "
Soil exposed,	"	$16\frac{1}{2}$ "	1·069 "
" under tray,	"	9 "	0·583 "

Next morning at 9 A.M.

		Grass.	Soil.
Temperature on surface,		39°·5	39°·5
" of soil,	3 inches under surface,	42°·5	40°·5
" "	12 " "	45°·5	45°·5

Grass exposed,	lost	19 grains, or	1·231 grammes.
" under tray,	"	13 "	0·842 "
Soil exposed,	"	30 "	1·944 "
" under tray,	"	18 "	1·166 "

These figures cannot be supposed to represent anything definite, they only indicate a condition of matters which has not been previously observed. They show that evaporation in our climate is going on night as well as day during dry weather, but the extent to which it takes place cannot be gathered from these observations, as they are far too few for the purpose—too few alike with regard to seasons, humidities, and exposures; nor can the proportionate amount of evaporation from bare soil and from grass-land be arrived at from the weights given. These readings can only be considered true for the place and moisture at the time of the year when the experiments were made. For instance, the inverted trays over soil in my early experiments always indicated a larger evaporation from soil than from grass, while the later ones did not. But the early experiments were made over soil freely exposed to sunshine during the whole day, while the later ones were made at a place less freely exposed, on account of the situation where the first experiments were made being too far from the place of weighing. It is evident the amount of sunshine will be an important factor in this nightly evaporation, as it will greatly determine the amount of heat stored up during the day, and available for evaporation during the night.

I extremely regret the season was so far advanced before these experiments were begun, as most of the weather suitable for the purpose was past. I have, however, endeavoured to check my results as well as possible. Still I feel that what has been done is only preliminary. Similar experiments would require to

be made during the whole year, to determine whether this evaporation is constantly going on or not in fair weather, and to determine its amount under different conditions. The varieties of soils, of humidities, and exposures are so great that an enormous number of experiments would require to be made to determine with any degree of accuracy the amount of evaporation that takes place from any large tract of land.

The temperatures of the soil and of the air during these experiments were not high, but we must remember they were taken in October. In summer we have to deal with much higher temperatures and greater vapour tensions, and therefore the possibilities of heavier dews. On the 18th August I find the temperature $\frac{1}{2}$ inch under the surface of the soil at 4 P.M. was 82°, at 3 inches underneath it was 72°, the temperature of the air being 66°. At 9 P.M., at 3 inches deep, the temperature was 60° under grass and under bare soil. The temperature *on* the grass was 45°, while a thermometer placed *on* bare soil was 52°. Next morning the temperature at 3 inches under grass was 56°, and at the same depth under soil 52°. The soil at 3 inches down had thus lost 20 degrees during the night, and that nearer the surface would have lost a good deal more. Much of this loss would be spent in evaporating moisture. On this occasion it will be noticed that at night the difference between the temperature on the surface of the grass and on the bare soil was as much as 7 degrees, and this easily explains why the ground kept dry while the grass got wet.

So far as my limited observations go, evaporation is constantly going on from soil under grass, but on a few occasions it was doubtful whether the reverse process had not taken place, and vapour got condensed on the surface of bare soil. On one or two occasions in autumn, I observed soil which had been dry the previous day to be damp in the morning. The soil had evidently received an increase of moisture. But the question still remains, Whence this moisture? Came it from the air, or from the soil underneath? The latter seems the more probable source, as the higher temperature below would determine a movement of the moisture upwards by the vapour diffusing; and the surface soil being cold, the vapour would be trapped by it before it escaped into the air, in the same way as it is trapped by grass on grass land.

During summer it is difficult to trace the vapour condensed on the surface of the soil to its source, and to say definitely whether it came from the air or from the ground underneath. But on the morning of the 12th October I had an interesting opportunity of studying this question. During the night the radiation had been very powerful, the surface of the soil was greatly cooled, and a thin crust of frozen earth formed. After the sun had thawed the surface it was very wet. An examination of the soil before the sun had acted on it, showed that the vapour condensed near its surface had come from under-

neath. On lifting the small clods on the surface, it was observed that their under surfaces and sides, when close to each other, were all thickly covered with hoar-frost so thickly as to be nearly white, while the upper surfaces exposed to the passing air had but little deposited on them,—the interpretation of which seems to be, that the vapour rising from the hot soil underneath had got trapped in its passage through the cold clods. Its presence underneath and on the sides of the clods was an evidence that the moisture was on its passage from the ground, when it met with the cold surface which imprisoned it.

This hoar-frost on the sides and under the clods could not be due to vapour condensed from the passing air, because the upper surfaces of the clods had scarcely any deposited on them, and that in spite of the fact that the upper surfaces would be the colder, as they were those from which the radiation was taking place. It seems probable that even the vapour condensed on the upper surfaces of the clods was part of the vapour escaping from the soil, and was not taken from the passing air.

The occasions when the earth is most likely to receive vapour condensed upon it from the passing air, are not on clear nights when the radiation is strong, but rather when after strong radiation and cooling of the surface the weather changes, becomes cloudy, and a warm moist wind blows over the land. Occasions of this kind are seen most frequently after frosts, and undoubtedly much moisture is then condensed on the soil, but the moisture so condensed is not what we call dew.

Dew on Roads.

There is considerable difference among works on dew as to the absence of dew on roads, but almost all agree in stating that it is never formed on roads; and the presence of dew on grass, while none is visible on roads, is generally attributed to the greater radiating power of vegetation over that of the material of which our roads are composed. Now I find that this statement as to facts is wrong, and the explanation is also inaccurate. Dew really does form on roads in great abundance on dewy nights, and the material of the road is practically as good a radiator as the grass.

The reason why it is generally said that dew is not seen on roads is owing, not to the less radiating power of the stones, but to the fact that dew has not been looked for at the proper place. The blades of grass are practically non-conductors of heat, while stones conduct fairly well. The result of this is that we are not entitled to look for dew on the upper surfaces of stones, as on grass, but it must be sought for on their under sides, because the stones are good conductors, and the vapour tension under them is much higher than at their upper surfaces, owing to the higher temperature of the air laden with

moisture rising from the ground. If we examine a gravel walk on a dewy evening, we shall find the under sides of the stones, especially those near the solid ground, to be dripping wet; and we may occasionally see isolated patches of stones wet on the upper surface, probably due to an openness in the ground at the place permitting a free escape of vapour.

Another reason why the upper surface of the gravel does not get wet, is that it is in good heat communication with the ground; the stones are thus kept warm; and as a good deal of the vapour rising from the ground is trapped by the under surfaces of the stones, the vapour which escapes these surfaces is not enough to saturate the air at the temperature of the exposed surfaces of the gravel. The following temperature, taken at 10 P.M. on the 25th September, will give an idea of the difference in temperature on the surface of grass and on gravel, and show why no dew is formed on the top of the stones while it collects on the grass. A thermometer placed on the surface of the gravel was 34°, while one placed near it, but on grass, was 30°, or 4° lower. At the surface of the soil under the grass the temperature was 40°, and it was almost exactly the same temperature at the bottom of the gravel which was $1\frac{1}{2}$ inches deep.

We see from the above that hot vapour, rising from the ground under grass, ascends till it comes into contact with the cold blades, and is condensed on their exposed surfaces; whereas on the gravel road the under sides of the stones are nearly as cold as their exposed surfaces, and much of the warm vapour gets condensed under them, while the vapour which escapes to the surfaces has its dew-point lowered by mixing with the surrounding air, and the upper surfaces of the stones being in good heat communication with the ground, are not cool enough to condense this vapour and form dew.

A simple manner of studying the formation of dew on roads is to take, say, two slates, and place one of them on the gravel and one on a hard part of the road. If these slates are examined on a dewy night, their under sides will be found to be dripping wet, though their upper surfaces and the road all round them are quite dry. This experiment also shows us that under most conditions of our climate vapour does rise from hard dry-looking roads on dewy nights.

In studying questions of this kind, and for showing the importance of the heat communicated by the earth to the radiating body, the following experiment may be useful. Place on the grass, soil, or road, a slate and a piece of iron, say an ordinary 7 lb. weight. Alongside of these place another slate and weight; but instead of the latter resting on the ground, elevate them a few inches on small wooden pegs driven into the earth. If we examine the surfaces of these bodies on dewy nights, the following will be the general result. While the grass all round is wet with dew, we shall find that the upper surfaces of the slate and the weight resting on the ground keep dry, and

those of the elevated ones get wet like the grass. the reason for this is that the bodies on the ground as well as the elevated ones are constantly losing more heat by radiation than they receive by absorption; but those in contact with the ground have heat communicated to them by conduction and by the condensation of vapour on their under surfaces; their temperature is thus prevented from falling as low as that of the elevated bodies, which only receive heat from the passing air; the latter are thus cooled more by radiation than those on the ground. Bodies out of heat communication with the ground thus tend to cool more than those in contact with it; and while the former get cooled below the dew-point and collect dew, the latter keep warmer than the dew-point, and thus tend to keep dry, or if wetted to become dry again.

These considerations suggest a simple method of testing whether the surface of any particular part of the ground is giving off vapour or not. It is very evident that so long as the temperature of the surface of the soil is above the dew-point of the air, vapour will rise from the ground, and that if the surface is cooled by radiation below the dew-point, evaporation will cease, and vapour will condense upon it. In order to test this, all that is necessary is to place on the ground, and in good heat communication with it, some substance that is a good conductor, and shows dewing easily. A piece of metal covered with black varnish does well. It is painted black, not in order to radiate copiously, but because black shows any deposit of dew most quickly and easily. So long as this test surface keeps dry while in contact with the ground, the soil round it must be giving off vapour, because the temperature of its surface is higher than the dew-point. But if the temperature of the ground falls below the dew-point it will collect moisture, and this test surface will collect dew also, and will thus tell us that the surrounding soil is receiving moisture. In experiments such as these we are simply converting a small area of the earth's surface into a condescending hygroscope, and our test surface tells us whether the earth's surface at the place is cooled by radiation below the dew-point or not. So long as no dew forms on the test surface vapour is being given off.

These test surfaces must not be large, at most only two or three centimetres, because if large they would check the free passage of the vapour to the air, and so prevent the soil under them from cooling to the same amount as the surrounding ground; and further, it is difficult to get good contact with large surfaces, without which only a part of the test surface keeps clear, while the part not in contact gets dewed, even though the temperature of the surface of the ground is above the dew-point. This was confirmed by observations made on a frosty night. On lifting each test plate, it was observed that the soil was frozen to it under the clear parts, and no soil adhered under the parts that were dewed. In my experiments I have used small copper discs covered

with black varnish, ordinary glass mirrors, and also small black mirrors, in order to get rid of the objection to ordinary silvered mirrors, namely, that they might not be good radiators. On no occasion up to the beginning of November have I yet seen dew on any of these at night, but it is difficult to say whether dew had not formed on them on some mornings, as the air was thick and misty, and the deposit then observed might have fallen as fine rain.

The changes in temperature of the surface of the soil due to radiation, give rise to a downward movement of heat during the day, and to an upward movement of it during the night. These heat changes will be accompanied by corresponding movements of the moisture in the soil. During day, after the surface is heated, the vapour tension being higher above than below, a downward movement of moisture will take place; and at night this process will be reversed, the tension of the vapour at a depth being greater than near the surface, the vapour rises and condenses in the colder soil. Part of the latent heat so liberated by the rising vapour is spent in radiation from the surface, part in evaporating moisture, and a little in heating the air cooled by contact with cold grass, &c.

We may conclude that, owing to the heat received during the day, and probably also to the internal heat of the earth, vapour continues to rise from the ground long after the sun has set, and in many conditions the vapour continues to rise the whole night; but under certain others it seems probable that the reverse will occasionally take place, and vapour condense on the ground. This is most likely to take place soonest on bare soil, especially on those parts of it that are in bad heat communication with the ground underneath. But over grass-land in most conditions of our climate, when dew is forming, the evaporation seldom seems to stop, but goes on night and day, on account of the surface of the soil being protected by the grass from losing its heat so quickly as the bare soil. The escaping vapour rises till it meets with some surface not in good heat communication with the ground, and which has been cooled by radiation, in the manner set forth by WELLS and others. These remarks refer to weather when dew is most abundant, as in spring, summer, and autumn, and do not apply to those conditions in which a warm vapour-laden air is brought over a cold ground.

DEW AND WIND.

It is well known that during windy nights no dew is formed. We previously knew that wind acts in two ways to prevent the formation of dew; to these two ways we must now add a third. Wind prevents the formation of dew—(1) by mixing the hot air above the surface of the ground with the air cooled near its surface, this tends to prevent the air being cooled to the dew-

point; (2) the wind by its passage over the surface of radiating bodies prevents these surfaces being cooled much below the temperature of the air; the wind thus tends to prevent the air in contact with these surfaces being cooled below the dew-point; and (3) wind blowing over the surface of the ground rapidly carries away the vapour rising from the soil, and mixes it up with a large quantity of drier air. The wind thus tends to prevent an accumulation of damp air near the ground.

To illustrate this third effect of wind, let us use the observations made on the evening of October 12. The sky was clear, and there was a considerable amount of radiation, but a slight wind was blowing. The bare soil in the test-pan lost 22 grains and the corresponding turf lost 30 grains in about five hours. Almost no dew was formed on the grass, but trays placed over the bare soil and over grass had their inside surfaces covered with moisture, though not so heavily as was generally observed on dewy nights. The reason why so little dew formed on this occasion was, partly, that the wind prevented the temperature of the air near the ground falling as much as it would have done if it had been calm. In the screen the temperature only fell to 40°. On the grass, however, it fell to 31°·5, and on the soil to 35°·5; but a good deal depended on the exposure of the thermometer to the wind. From the above we see that, though wind was blowing, the thermometer on the grass fell a good deal below the temperature of the air, and showed a considerable amount of radiation. The wind apparently prevented the formation of dew on this occasion, principally by preventing an accumulation of moist air near the surface of the ground. The inverted trays showed that if the wind had fallen dew would have formed, because it formed in the still air under the trays. The deposit was not so heavy inside the trays on this occasion as was often seen in dewy nights, because the wind prevented the radiation cooling the top of the trays to the same extent as when it was calm.

Dew and Vegetation.

When I began to make observations on dew, one of the first things I did was to make a tour of the garden on a dewy night, and to examine the appearance of the plants. A very short survey was sufficient to show that something else was at work than radiation and condensation to produce the effects then seen. Let me briefly describe what I saw, and what at once struck me could not be explained by the ordinary laws of radiation and condensation. Certain kinds of plants were found to be covered with moisture, while others were dry. Many plants of the *Brassica* family were heavily covered with glistening drops; while beans, peas, &c., growing alongside them, were quite dry. Again, in clusters of plants of the same kind some were wet, while others were not; and not only so, but some branches were wet, while

others on the same plant were dry. These differences were noticed to be quite irrespective either of their exposure to the sky, or to the probable humidity of the air surrounding them.

In illustration of this latter point, small clusters of dwarf French poppies may be mentioned. Most of the plants were quite dry, whilst others growing amongst them were dripping with moisture; and while some branches were dry, others on the same plant were studded with drops, and the general surface of the leaves in some cases wet. On examination of these plants next day, it was observed that those that were wet at night were all plants in vigorous growth, and the shoots that were dewed were those in which the vegetation seemed most active. It was also observed that it was always the same plants and branches that were dewed night after night during the short time the observations were made.

A closer examination of the leaves of broccoli plants showed better than any others that the moisture collected on them was not deposited in the manner we should expect if it had been deposited as an effect and according to the laws of radiation; nor was it deposited in accordance with the laws of condensation; indeed, every appearance was at variance with these laws. Examination showed that the moisture was collected in little drops placed at short distances apart, along the very edge of the leaf, while the rest of the leaf was often dry. Now, if the moisture had been condensed by cold produced by radiation, then it would have been most abundant on the upper surface of the leaf; but there would have been none on its windward edge. This is well seen when we expose a small glass plate on a dewy night; the windward edge is always dry, and the deposit is spread evenly over the rest of the plate up to the opposite margin, because the temperature of the air when it first strikes the plate is higher than the dew-point, and it has to travel over more or less of the surface of the glass before it is cooled enough to deposit its moisture. Again, if these drops on the edge of the leaf had been deposited according to the laws of condensation, then the moisture would have been deposited on the surface more in accordance with the distribution of temperature at the different points; the moisture would therefore have been more equally distributed, and not been in large isolated drops.

On further examining these plants, I placed the lantern behind the blade, and then observed that the position of the beautiful sparkling diamond-like drops that fringed its edge had a definite relation to the structure of the leaf; they were all placed at the points where the nearly colourless and semi-transparent veins of the leaf came to the outer edge, at once suggesting that these veins were the channels from which the drops had been expelled.

These isolated drops on the edges of the leaves were therefore evidently not dew, but an effect of the vitality of the plants. An examination of grass

blades showed that they also tend to have large drops attached to them, while the rest of the blade is dry, and these drops were always found to be situated at certain definite points; they were always near the tips of the blades. *These large drops* seen on plants at night are therefore *not dew* at all, but are watery juices exuded by the plants.

Now this excretion of water by the leaves of growing plants is not a new discovery—it has been long well known. But what seems extremely curious is, that its relation to dew has never been recognised, at least so far as I am aware, and it must be admitted that it is one of considerable importance.

It is well known that plants transpire from their leaves an immense amount of moisture, which passes off in an invisible form. Prof. J. Boussingault found that mint transpired 82 grammes of water per square metre in sunshine, and 36 grammes in shade; but if the roots of the plants were removed, they only transpired 16 and 15 grammes respectively. This simple experiment proves that the root sends into the stem of the plant a supply of water, that it acts as a kind of force-pump, and keeps up a pressure inside the tissues of the plant. This supply sent in by the root is in most conditions removed by means of transpiration from the surface of the leaves.

Now what will be the result if transpiration is checked, while the root continues to send forward supplies? It will evidently depend on two things—first, the pressure the root is capable of exerting before its action is stopped; and second, the freeness with which the water can escape from the leaves. If the root pressure is small, it will cease with the transpiration; but if it is great, the sap will be forced into the plant, and if nature has provided any outlets it will escape at these openings.

Dr J. W. Mool* has given great attention to the subject, and has experimented on a number of plants. The method he employed in his researches was to place the leaves under the most favourable conditions for the excretion of drops, by diminishing the transpiration as far as possible, and by supplying them with water. He substituted for root pressure, a pressure produced by a column of mercury. Out of 60 plants experimented on by Dr Mool, he found that the leaves of 29 excreted drops without being injected, 13 leaves became injected and excreted drops, and 18 became injected and did not excrete at all. He says that the excretion takes place by water-pores, and by ordinary stomata, while in some cases it occurs at surfaces possessing neither of these organs.

I have recently made a few experiments on this subject in its relation to dew. As, however, the season was far advanced before the experiments were begun, but little could be accomplished, for the activity of the plants was nearly over, and grass was almost the only plant possessing sufficient vitality for

* *Nature*, vol. xxii. p. 403.

experimenting. I however removed a branch of the poppy, which, during summer, had shown such a tendency to exude moisture, and connected it by means of an india-rubber tube with a head of water of about one metre. After placing a glass receiver over it, so as to check evaporation, it was left for two or three hours, when it was found to have excreted water freely—some parts of the leaves being quite wet, while drops had collected at other places.

The broccoli plants which had excited my interest in summer were also experimented with. A full-grown leaf was fitted into the apparatus, and the pressure applied. In a little over an hour it also exuded water, and soon got fringed with drops along its edge in exactly the same way that was observed on it in summer. Another leaf from the same plant, but much younger, being about one quarter grown, on being tested in the same way did not excrete at all, after the pressure had been applied for twenty-four hours. Here we have the same result as that noticed in summer—one leaf exudes, while another on the same plant does not.

If the water pressed into the leaf is coloured with aniline blue, the drops when they first appear are colourless, but before they grow to any size, the blue appears, showing that little water was held in the veins, but the whole leaf got coloured of a fine deep blue-green, like that seen when vegetation is very rank, showing that the injected liquid had penetrated through the whole leaf.

Most of my experiments on this subject were made with grass. I find that even in the middle of October, after having been severely frosted two or three times, which had probably reduced its vitality, it still exuded so abundantly that drops collected in air which was not saturated. A turf placed in a cellar, dry enough to keep glass quite free from dewy deposit, soon collected drops. These drops always appear near the tips of the blades; they are not exuded from every blade, and sometimes from only one on each stalk, but generally from more; and it is always from the blades that seem to have the greatest vitality, and are nearly, but not quite full grown. Sometimes it is the youngest blade that exudes, but if it is very small, it is the second youngest. As the blades grow old they cease to exude; but this seems to be due to some change in the blade at the point where it exuded, and not to a diminution of root pressure, as it exudes freely when the tip is cut off.

The question might be here raised, Are these drops really exuded by the plant? Are they not due to some condensing power possessed by the leaves, by the presence at these points of some substance possessing an affinity for water vapour, or some process by which they may extract moisture from the air? To get an answer to this question, I selected a small turf, placed over it a glass receiver, and left it till drops were excreted. Removing the receiver, a blade having a drop attached to it was selected. After being

carefully dried, the tip of the blade was placed in a small glass receiver, so as to isolate it from the damp air of the larger receiver. This small covering glass measured about 10 mm. in diameter by about 15 mm. in height. Its open end was closed by means of a very thin plate of metal cemented to it. In the centre of this plate was pierced a small opening, of the same size and shape as the selected blade of grass. The tip of the blade was entered about 5 mm. into this small receiver, and to prevent moisture entering and coming in contact with the tip of the blade, an air-tight joint between the blade and the metal was made with india-rubber solution. The tip of the blade was thus isolated inside the small receiver in which the air was dry. The large glass receiver was then placed over the turf to prevent evaporation from the lower part of the blade, or the experiment was made in a room where the air was not very dry. After a time, generally some hours, the turf was examined. A drop was always found to have formed on the tip of the blade inside the small receiver, and this drop was, as nearly as could be judged, always as large as the drops formed in the moist air under the large receiver. It would thus appear that these drops are really exuded by the plant, and not extracted from the air.

These exuded drops seem to be almost entirely the result of root pressure, because if we cut off the roots, and place the stems in water, putting over all a glass receiver standing in water so as to saturate the air, and as a test that the conditions are favourable, placing a small turf alongside the cut grass under the receiver, we shall find that scarcely any drops make their appearance on the rootless stems, while those with roots have drops attached to them. Again, if we take one of these rootless stems, and attach it by means of the india-rubber tube to a head of water, it is found to exude drops at the tips of its blades in moist air in the same way as when it was attached to its roots.

These excreted drops are formed on grass on other than dewy nights. After rain, if there has been no wind, and the air near the ground becomes saturated, a rearrangement of the drops takes place. Some time after the rain has ceased, most of the blades will be found to be tipped with a drop at the same point as the exuded drop appeared at night—a position which no falling rain drop could keep. This tendency of plants to exude moisture explains why the grass is almost always wet during autumn. At that season evaporation is slow, and as the plants are constantly pouring in supplies to the drops, it takes a long time for the slow evaporation to overcome the wetting effect and dry up the grass.

The question as to what degree of humidity in the air is necessary before plants will exude drops, would seem to be greatly determined by the rate at which the supply is sent into the leaf. If the supply is greater than the

evaporation from the whole surface of the leaf, the drop grows; but if the supply is less, it does not form, or if formed, it decreases in size. The rate of the supply will evidently depend on the kind of plant and the amount of its vital activity at the time. The formation of drops on plants that exude moisture will therefore depend on the rate of supply, the humidity of the air, and the velocity of the wind. It is not easy to get a satisfactory experimental answer to this question, on account of the soil near the grass tending to moisten the air over it. A small turf placed in an elevated position in the centre of a room has been observed to have drops on it, when there was a difference of more than one degree between the wet and the dry bulb thermometer hung alongside. As the drops are exuded at the tips of the blades, it is probable the air in contact with them was not much moistened by the small area of soil underneath.

These observations entirely do away with the explanation usually given of the tendency of grass to get wet early and heavily on dewy nights. It has generally been explained by saying that grass is a better radiator than most substances, and therefore cools more, and sooner, than other bodies. We now see that those drops that first make their appearance on grass are not drops of dew at all, and their appearance depends, not on the laws of dew, but on those of vegetation. Hence the varied distribution of moisture on plants and shrubs on dewy nights.

We have seen that much of the moisture that collects on plants at night does not form like dew on dead matter. Dead matter gets equally wet where equally exposed, and the moisture does not collect on it in isolated drops, as it does on plants. Those drops which appear on grass on clear nights are not dew, and they make their appearance on surfaces that are not cooled to the dew-point. If the radiation effect continues after these drops have been forming for some time, true dew makes its appearance, and now the plants get wet all over their exposed surfaces in the same manner as dead matter. This latter form of wetting or true dew is of rarer occurrence than we might at first imagine. On many nights on which grass gets wet, no true dew is deposited on it; and on all nights, when vegetation is active, the exuded drops always make their appearance before the true dew; so that when we walk in early evening over the wet lawn, it is not dew that we brush off the grass with our feet, but the sap exuded by the plant itself. The difference between these exuded drops and true dew can be detected at a glance. The moisture exuded by grass is always excreted at a point situated near the tip of the blade, and forms a drop of some size, which may form while the rest of the blade is dry, but true dew collects evenly all over the blade. The exuded liquid forms a large glistening diamond-like drop, whereas dew coats the blade with a fine pearly lustre.

I feel that the dissecting hand of science has here done an injury to our poetic feelings. Every poet who has sung of the beauties of nature has added his tribute to the sparkling dew-drop, and BALLANTINE in his widely-known song has taught a comforting lesson from the thought that "ilka blade o' grass keps its ain drap o' dew." No doubt the drop of dew to which the poets refer is the large sparkling diamond-like gem that tips the blades of grass, and which we now know is not dew at all. While, however, our interpretation of nature has changed, the teaching of the poet remains, and the sparkling dew-drop may still teach the same comforting lesson. We must, however, change our views regarding the source of the refreshing influence. We may no longer look upon it as showered down from without, but as welling up from within—no longer as taken by the chill hand of night and given to refresh and invigorate exhausted nature; we must rather look upon it as suggesting that we are provided with an internal vitality more than sufficient to restore our exhausted powers, after the heat and toil of the day are past.

RADIATION.

I have said in a previous part of this paper that the surface of bare soil and of roads will radiate at night as much heat as grass. It may be thought I have said this simply because we do not now require that grass should be the more powerful radiator to enable us to explain its greater wetness on dewy nights. Though it is not now necessary to suppose that grass is a powerful radiator, yet there is nothing in the above experiments to prove it either a good or a bad one. It therefore seemed desirable that some definite experiments be made on this point, and also to determine the radiating powers of different substances at night, as this is always an interesting and important point in questions connected with the deposition of dew; and the radiating power of grass, though not the principal cause of its wetness at night, might be still considered to play a subordinate part.

We have already a great number of experiments on the radiating powers of different substances. Unfortunately most of the accurate measurements of this kind are from laboratory experiments, and do not appear to bear very directly on our subject. FRANKLIN'S early experiments, made with different coloured cloths placed on snow, seem to have given our ideas an unfortunate bias on this subject. From observing the different depths to which cloths of different colours sunk in snow, when exposed to solar radiation, he came to the conclusion that the dark colours absorb most heat, and this conclusion seems for long to have influenced our ideas. If the heat radiated and absorbed by a surface was composed entirely of visible rays, then no doubt the colour of a body would be an index of its radiating and absorbing powers.

But as the eye gives us no information about the greater proportion of the radiant energy, its indications are of no value in determining the radiating and absorbing powers of different surfaces.

Experiment shows that different surfaces have different absorbing powers for different rays. Melloni, for instance, found that white lead absorbed only about half as much heat from a Locatelli lamp as lamp black did, while it absorbed as much as lamp black when the source of heat was copper at 100° C. It is evident from this, that we cannot take the result of experiments made in the laboratory, and apply them to surfaces exposed to the temperature of the sky on a clear night. It may be possible that the radiating and absorbing powers of different surfaces may bear the same proportion to each other when the temperature is 0°, and they radiate into space, as when their temperature is 100°, and they are exposed to surfaces at the ordinary temperature of the laboratory. This may be so, but till it is proved we cannot apply these laboratory experiments to the cooling effect of radiation at night.

Some experiments on the radiating power of different substances exposed to a clear sky were made by Daniell. He used for his purpose two similar parabolic reflectors. In the focus of each was placed the bulb of a thermometer. In experimenting he turned the reflectors to the sky, and coated the bulbs of the thermometers with the substances to be tested. Comparing garden mould with black wool, his measurements show, from the average of three readings given by him, that while the black wool fell 9° below the temperature of the air, the mould fell only 6°. The difference between the radiating powers of chalk and black wool, as given by him, was not quite so great. There seems to be an objection to this method of experimenting. The different surfaces here lose more heat by radiation into space than they receive. To supply this loss, they receive more heat by radiation from the reflector than they give, and they also receive heat from the surrounding air, conveyed to them by connection currents. Now in the experiment as arranged by Daniell, the two surfaces will not receive the same amount of heat from the latter source. The wool surface will not have such a free circulation of air over it as the other one; it will therefore not receive so much heat, and its temperature will thus tend to fall lower.

It appeared that something more might be done in this direction, and on consideration it was thought that the radiation thermometers, described by me in a previous paper, might be suitable for the purpose. It may be remembered that the principle on which these radiation thermometers is constructed is, that a large surface is more highly heated than a small one by radiation during the day, on account of the absorbed heat being more slowly taken away by the passing air from the former than from the latter; and for a similar reason

a large surface is colder at night than a small one, as the small surface receives more heat, per unit of area, from the air than the large one. The absorbing and radiating surface of these instruments is a *large* flat area, painted black, and its temperature is taken by means of a thermometer, with its bulb placed under the centre of the radiating surface.*

The construction of these radiation instruments has been altered, and those used in this investigation were made of metal in place of wood, as described in the previous paper, the radiating surface being a thin plate of metal, 14 inches (355 mm.) square. A thin metal tube is fixed close to and parallel with the under surface of the plate. One end of the tube terminates at the centre of the plate, and the other at the edge. The thermometer is placed in this tube with its bulb under the centre of the plate, and to prevent heat escaping or being absorbed at the back, a considerable thickness of cotton wool is placed under it. The instrument is practically a shallow box, 14 inches square by 2 inches (51 mm.) deep, packed with cotton wool. One of the flat areas of the box is exposed to radiation, and its temperature is taken by means of a thermometer placed under its surface. In the following I shall refer to this instrument simply as the thermometer box.

One of the advantages of this form of instrument for solar radiation experiments is, that the readings given by different instruments agree with each other, at least this is the case so far as my experience goes; and it is well known that the vacuum radiation thermometers are unsatisfactory in this respect, no two almost ever reading alike. For instance, the vacuum radiation thermometers used at the Indian Stations, when compared with another of the same pattern as standard, were in some cases found to differ as much as 15°, though they were exact copies of each other, and similarly exposed.† I find that when the different instruments of the kind used by me are compared they agree very well when of the same size. It is of course necessary that they be of the same size—this results from the principle of their construction. It seems possible that we might make boxes of different sizes, and from them determine the law of variation for size; so that, knowing the size of the surface used in any particular set of observations, we could determine what temperature its readings corresponded to in another instrument of a different size, or all readings might be reduced to a standard size, say the temperature of a very large surface.

I may mention that the temperature given by an instrument of the size here described when placed in sunshine is a good deal above that indicated by a vacuum thermometer, which had been carefully prepared for me by Casella of London. Generally the readings were about 12 per cent. higher.

* Thermometer Screens, *Proceedings of the Royal Society, Edinburgh*, No. 117, 1883–84.

† Report of the Meteorology of India, 1879, by H. F. Blanford, F.R.S.

One objection to these large-surface radiation thermometers is that they are more affected by wind than the vacuum ones. If it is a question of solar energy we are considering, this certainly is an objection, but if it is one of climate it will scarcely be so. I need not say that for questions of terrestrial radiation at night the vacuum thermometer is of no use.

In using these thermometer boxes for determining the radiating powers of different surfaces at night the following method was employed:—Two precisely similar boxes were prepared, and their upper surfaces painted black. They were placed in an elevated position in the open air, commanding a clear view of the sky all round. They were first exposed without anything on their surfaces, to see if their readings were exactly alike. In constructing them care was taken to put the same amount of cotton wool in each, in order that their non-conducting powers and heat capacities might be the same, so that both might take the same amount of heat to warm them, and both lose the same amount of heat at the back. On trial both instruments were found to read alike when similarly exposed.

As the sky radiation is a rather variable quantity, it would not do, on most nights, to leave one of these test surfaces bare, and use it as a standard with which to compare the other, over which we have put the substance to be tested, because the uncovered surface will follow the changes in the radiation more easily than the other, and will change more, and sooner, than the one covered with the substance to be tested, particularly if the substance is a bad conductor. The method generally adopted was to place both surfaces as nearly as possible under the same conditions. For instance, the first substances tested were black and white cloths of different materials; of each kind a black and a white was selected, each pair being as much alike as possible, of the same material, of the same weight, and of the same texture. A black one was placed over one thermometer box, and the corresponding white one over the other. After a time the readings were taken, and the position of the cloths reversed, the black being placed over the box where the white was, and *vice versa*, and readings again taken. Then if radiation remained constant one of the cloths was removed, and the other compared with the black surface.

The following table shows the results of some experiments made on the radiating power of black and white cloths tried in this way. The readings were taken on the evening of the 14th November. The sky on the occasion was quite cloudless. The air was very dry, and had scarcely any movement—an unusually favourable condition for conducting experiments of this kind. The radiating surfaces were placed at a height of about one metre from the ground, and a protected thermometer for taking the temperature of the air was placed alongside at the same height.

Air.	Substance.	Radiation.	Substance.	Radiation.
35°	No. 1, black	28°	No. 1, white	28°
35°	„ white	28°·5	„ black	28°·5
35°·5	„ „	28°·5	„ „	28°·5
35°·5	Paint black	28°·5	„ „	28°·5
36°	No. 1, black	28°·5	Paint black	28°·5
36°	No. 2, „	29°	No. 2, white	29°
35°	„ „	29°·2	„ „	29°·3
35°	„ white	29°	„ black	29°
35°·5	„ „	28°·7	„ „	28°·8
35°·5	„ „	28°	Paint „	28°
35°	No. 3, white	27°	No. 3, black	27°
35°	„ black	27°	„ white	27°
34°·5	Paint „	26°·5	„ „	26°·5
34°·5	„ „	26°·5	„ „	26°·5

In the above table, the first column shows the temperature of the air at the height of the radiating surfaces. In the second and fourth columns are the substances whose radiating powers are compared, No. 1 being black and white cotton cloths, No. 2 merino cloths, and No. 3 thick woollen cloths. In the third and fifth columns are the temperatures of the radiating surfaces. The following was the manner of conducting the experiments :—Take the first on the table. A black cotton cloth was spread over one thermometer box, and a white cotton one over the other; after a time, when the readings were taken, the temperature of the air was 35°, the black cloth 28°, and the white one 28°. The black cotton was now removed from its box, the white one put in its place, and the black one where the white one previously was. This was done to check any error from difference of exposure to wind or difference in thermometer boxes. After a time the readings were taken, and found to be—air 35°, white cotton 28·5, and black cotton 28·5. The radiation of the cloth was then compared with the radiation from the black paint on the surface of the radiation box. This was possible on this night, as there was no wind, and radiation was fairly constant.

In my first experiments with black and white cloths, they were found to be cooled to an unequal amount; but as the cloths used on this occasion were what first came to hand, and happened to be of unequal thickness and texture, and as there was wind blowing at the time, the heating effect of the passing air acted unequally on the different cloths, and prevented them from being cooled to the same amount; hence the necessity of using cloths of equal texture in experiments of the kind, especially when wind is blowing.

It will be observed that these experiments do not show any difference in the radiating powers of white and black cloths ; nor do they show any difference in the radiating powers of cotton, wool, and paint. All radiate equally well,

and have their surfaces cooled to the same amount when exposed to the same radiation. It will be noticed that the temperature of the radiating surfaces varied during the experiments, and was from 6 to 8 degrees below the temperature of the air.

These experiments make no claim to any great degree of accuracy; the conditions under which they are made make it difficult to get correct results, as the readings have to be taken with the aid of a lantern in the open air on cold nights, and as special thermometers had not been prepared for the radiation boxes, the thermometers used had to be partly withdrawn from the boxes before reading; there may therefore be a slight inaccuracy in the temperatures given. The error from this cause is not likely to be more than a quarter of a degree, and if there had been any great difference in the radiating powers of the surfaces, it would have shown on a scale of 6 to 8 degrees.

The following table gives the result of a comparison made between the radiating powers of grass and garden soil, on a calm evening when the air was dry. One of the thermometer boxes was sprinkled over with the soil, and over the other was put a layer of cut grass just sufficient to conceal all the black surface, and pressed down so as to make as flat a surface as possible:—

Temperature of air.	Temperature of grass.	Temperature of soil.
34°	25°·5	25°
35°	27°	26°·5
35°	27°	26°
35°	27°	26°·5
35°	27°	26°·5

From the above it will be seen that the garden soil was colder than the grass on this evening. When the grass was removed from the box and the soil compared with the black paint on the other box, the soil was found to be a little colder than the black paint, but not so much as it was colder than the grass. The reason for the soil being colder than the black paint would appear to be due to the evaporation taking place from its surface; the dew-point at the time was very low, and the top of the soil showed signs of drying. Compared with grass, this was not the reason for the difference, as the grass was slightly damp. The higher temperature of the grass would rather appear to be due to the nature of its surface. The passing air would communicate more heat to its irregular surface than it would to the more even one of the soil. Grass and soil were compared on other evenings on which the air was not so dry, and the exposed surfaces had vapour condensed on them; on these occasions the two surfaces radiated almost equally well.

This comparison of the radiating powers of grass and soil gives no support

to the idea that the greater wetness of grass on dewy nights is owing to its greater radiating power. The radiating powers of the two surfaces seem to be practically the same; and if neither grass nor soil received heat from the ground, the soil would cool lowest, because the grass in its natural condition would get more heat from the passing air, on account of its surface being irregular and in small pieces, as we know that small surfaces receive from this cause much more heat, per unit of area, than larger ones, this being particularly the case when there is wind. From which we see that the smallness of the blades in grass is really an advantage, and prevents their surfaces being cooled by radiation so much as they would be if they were larger.

The number of substances tested for their radiating powers at night is not so great as was hoped for, on account of the rare occurrence of evenings on which work of this kind can be done in this climate; for not only must the sky be free from passing clouds, in order that the amount of radiation may be as constant as possible, but the air must also be very dry, in order that the dew-point may be lower than the temperature of the cold radiating surfaces. If vapour gets condensed on the radiating surfaces, the radiation from the film of ice, or water, will interfere with the results. In making the experiments, a large sheet of glass was generally exposed alongside of the radiation boxes, to show if vapour was being deposited on the radiating surfaces. But even with this precaution we cannot be sure we are testing the radiating powers of some substances experimented on, because some kinds of matter have an affinity for water, and condense vapour on their surfaces from unsaturated air. As an example of uncertain results, I may mention a comparison made between salt and sugar. These two substances have been found by other observers to radiate very unequally at 100° C.—sugar radiating twice as much as salt. When tested at night, they were found to radiate equally well; but as both substances have an affinity for water, their surfaces would have a film of moisture over them, which would increase the radiating power of the salt, and thus make the test of no value.

Among the few substances that have been found to radiate less heat at night than a black surface is sulphur. On the night of the 7th December, when the air was very dry and the glass plate kept undewed, the following readings were taken:—

Temperature of air.	Temperature of black surface.	Sulphur.
27°	21°	23°
26°	19°	21°·25

The sulphur was sifted over the one thermometer box and the other left bare.

It will be observed that the black surface radiated one half more than the sulphur. This experiment suggests that a sprinkling of sulphur might be used as a protection to delicate plants on frosty nights, but whether it would pay or not experience alone can determine.

Polished tin was also tested, a sheet of tin being placed over one box, and another sheet painted black put over the other, so as to make the conditions of both boxes similar. The amount radiated by the tin was small; when the temperature of the black surface fell 7°, the tin only fell about 1°, more or less, according to the perfection of the polish of its surface.

For meteorological purposes the following observations made on the radiating power of snow will be useful. I regret that owing to the absence of snow so far this winter, I have only had one opportunity of making observations on this substance. In the following table will be found the readings given by the thermometer boxes, one of which was left bare, and gave the radiation of black paint, while over the other was put a thin layer of snow. This was done on the forenoon of the 10th December, and readings were begun shortly after mid-day, and taken from time to time till evening:—

Hour.	Air.	Black.	Snow.	Difference.
12·30 P.M.	28°	24°	21°	−3°
1 „	28°	24°·5	22°	−2°·5
2 „	26°·8	22°	20°	−2°
5 „	23°	15°·5	16°	+0°·5
5·30 „	21°	15°	15°·5	+0°·5
6·30 „	21°	13°	13°·5	+0°·5
8 „	19°	9°	10°	+1°

In the above table, the first column gives the hour at which the temperatures were taken. In the second column are the temperatures of the air; in the third are the temperatures of the black radiating surface; in the fourth are the temperatures of snow surface; and in the fifth column are the differences between the temperature of the snow and the black surface at the hour the readings were taken. The day on which this comparison was made was fine, clear, calm, and frosty, with the sun shining brightly. The radiating surfaces had a clear view of the sky, but were protected from the direct rays of the sun.

It will be observed that while the sun was high the snow surface was very much colder than the other; while the black surface only fell 4° below the temperature of the air, the snow fell 7°. As the day advanced, and the sun sunk towards the horizon, this difference decreased to 2°·5 at 1 o'clock and to 2° at 2 o'clock; and at 5 o'clock, by which time the sun had set, the snow

was a little warmer than the black surface, a condition in which it remained during the evening.

The reason for the snow being colder than the black surface during the day would seem to be, that both surfaces radiate and absorb "dark heat" about equally well, both surfaces therefore throw off about the same amount of heat; but while this is the case, their absorbing powers for the heat of the sun are very different, and though the sun was not shining directly on the surfaces, yet there is a considerable amount of its heat reflected to the surface of the earth from the atmosphere overhead. Now a black surface absorbs most of this reflected heat that falls upon it, while the snow absorbs very little. Hence, while both surfaces are radiating about the same amount of heat, the black surface is absorbing far more than the snow, and thus keeps warmer. As the sun sinks, the amount of its heat reflected by our atmosphere gets less and less, and the difference in the temperature on the two surfaces diminishes; and when at last the sun is quite under the horizon the temperature of the two surfaces becomes nearly equal. It will be, however, observed, that they never become quite the same, the snow being generally about half a degree warmer than the other. The whole of this difference is not, however, owing to difference in radiating powers; the snow will tend to give a slightly higher reading on account of its surface being rougher than that of the paint, thus causing it to receive more heat from the passing air than the black surface. And, further, from the conditions of the experiments, the readings being taken during a falling temperature, and the snow not being a good conductor of heat, the thermometer under it will take longer to fall than the one under the blackened metal. From these conditions it seems probable that there is not much difference between the radiating and absorbing powers of snow and black paint at night, while the difference is very considerable during the day.

It has been suggested to me that this difference in the radiating and absorbing powers of snow and black surface, such as soil, &c., will enable us to explain a difficulty long felt, regarding the hour at which the diurnal variation of temperature begins in countries covered with snow. Over those parts of the surface of our globe, where there is no snow, the temperature of the air begins to rise *before* sunrise, whereas in snow-clad regions this change does not take place till the sun is above the horizon—the explanation would appear to be that where the surface of the ground is dark it absorbs the heat of the sun, and warms the air whenever the rays begin to shine into the air overhead; but where covered with snow it is but little warmed by these early reflected rays, and it is not till the sun gets higher and shines on the surface of the earth that its effects begin to be felt.

General Remarks.

We see as a result of these experiments, that in our climate at least, water vapour is almost constantly rising from the ground, and this takes place from fallow land, from grass land, and from roads, even on nights on which there is heavy dew. There seems to be but little doubt that the tide of vapour almost always flows outwards from the earth, and ebbs but rarely, save after it has been condensed to cloud and rain. The question as to whether any surface is in a condition to lose or gain moisture on a dewy night depends on its more or less perfect heat communication with the earth. Those surfaces, such as soil, rock, stones &c., which are in good heat communication with the earth, tend to keep warm, and to lose moisture; while those surfaces not in good communication with the earth, such as leaves of plants, roofs of sheds, &c., tend to lose their heat, and gain moisture. This is the reason why grass tends to collect true dew, while stones on the ground remain dry. Grass is a bad conductor, and forms a non-conducting layer over the ground, preventing the earth from losing its heat. The inside of this covering is hot by contact with the earth, while its outside is cooled by radiation; and as the grass is a bad conductor, its exposed surface gets cooled by radiation to a lower temperature than the better conducting soil and stones; hence the appearance of dew on it, while the earth is dry.

Since vapour is constantly rising from the earth on dewy nights, it follows that any measurements of dew we may make ought not to be added to the rainfall, as the water so collected is in no sense a measure of the moisture returned to the earth at night, nor is it even a proof that any water is then returned. The amount of dew measured is simply a somewhat rough indication of the amount of moisture received by plants and other bodies not in heat communication with the ground; while the ground itself does not receive any, but is rather giving off vapour.

Dew is most copious during clear weather, and these experiments show us that this condition of weather has a threefold action in the production of dew—first, cloudless skies are necessary at night, in order that radiation may be strong, and the surfaces of bodies cooled low enough to condense the vapour; second, clear skies are necessary in order that a copious evaporation may take place under a hot sun during the day; and third, the same conditions are necessary that the ground may be highly heated by the sun, and a large amount of heat stored up during the day to be spent in evaporating an abundant supply of vapour during the night.

What are known as radiation fogs are generally supposed to be due to cold air flowing down, at evening, from higher levels to lower and warmer ones, and the mixing of the airs resulting in a foggy condensation. The more

probable explanation now seems to me to be, that they are caused by the uprising of the hot air and moisture from the ground, mixing with the colder air above the grass, much in the same way as a fog is produced over a river in sunny weather when the water is warm. So far as my observations go, these fogs generally form over flat damp fields, after hot sunny days, and they have been seen where there was no high ground from which cold air could flow in.

There almost seems reason for supposing that much of the moisture collected on grass, and which looks like true dew, may under many conditions be fine rain from fog formed in the manner above described. Because the hot air and vapour rising through the grass will tend to form fog, where it mixes with the cold air near the upper part of the grass, and if there is little wind this fog will settle on the blades. Under most conditions this fog will not form above the grass, and will not therefore be visible. It will often not form above the blades, because the hot moist air may there meet with too much dry air to supersaturate it, but there seems reason for supposing that it will be often formed amongst the stems of the grass.

During frosts we have excellent opportunities for studying the condensation of the vapour of our atmosphere, because it remains in the position where it is condensed and is easily seen, being neither absorbed by the ground nor dropped from the plants, &c., on which it may be deposited. I took the opportunity afforded by two nights of this kind for observing two opposite conditions of the air, and I shall here describe the effects of the radiation on the nights of the 14th and 15th November. During the afternoon the canopy of clouds that had hung over the earth for some days was gradually drawn aside, and moved away southwards. By 5 P.M. on the 14th the sky was cloudless. There was only a very slight movement of the air from the north, the radiation was strong, and the air dry. These conditions continued all night, and the minimum thermometer in the screen fell to 25°.

Next morning the ground and the grass were frozen. It was what is called a black frost. There was no hoar-frost on the trees, and what little there was on the grass was irregularly distributed. All the little hollows, of about a foot square in area and under, had a deposit of hoar-frost, while the higher parts of the grass had none. As there was no wind, and only a slow movement of the air, this peculiar distribution would not be caused by the heating effect of the passing air on the higher and more exposed blades, but was probably owing to its dryness. The small hollows being less freely exposed to the circulation, the air in them became more moistened from the vapour rising out of the ground than the air a little higher up, where it got mixed with a larger amount of the dry air. The test surface on the ground was quite dry at 9 P.M., and also next morning, showing that the ground had been giving off vapour all night.

In contrast with this, let us now look at the condition of matters on the

following morning. During the whole of the 15th the air remained calm and frosty, and the cold intensified during the night. On the morning of the 16th the minimum thermometer indicated a temperature of 19°. On this occasion we had a hoar or white frost. Grass, fences, shrubs &c., were all white, and the trees even to their top branches. The air on this occasion had evidently got cooled to near its dew-point, and moisture had condensed on almost every exposed surface, causing nature to present a remarkable contrast to its appearance on the previous morning, though both mornings were frosty.

We shall now refer in more detail to some of the points most worth noticing on these mornings. It was observed that the distribution of the hoar-frost on the grass on the morning of the 16th was the reverse of what it was the previous morning, the high blades on this occasion having rather a thicker coating of hoar-frost than those lower down. The reason for this was that the higher blades were exposed to the passing air, which on this occasion was saturated; whereas those on the hollows had to depend for their supply on what rose from the ground, which could not be much under the conditions, as the bottom of the grass and the top of the soil were cooled below the freezing-point, and most of the rising vapour would be trapped before it reached the surface. We may also note here that the test surfaces on the soil were quite dry, and that the slate and iron weight resting on the grass were free from deposit; while the elevated slate and weight, the grass, and almost everything else had a coat of hoar-frost, showing that the ground kept hot enough to give off moisture. An examination of the bare soil also showed that at most parts of its surface vapour was being given off. Wherever the contact with the ground underneath was good, no hoar-frost formed, and it was deposited only on the small clods that were lying on the surface; of course, there was plenty of hoar-frost on the under sides of the large clods, but none on their upper surfaces.

The slates and weights resting on the grass were frequently examined while the frost lasted, which it did till the morning of the 19th. During all this time the radiation was great, and the temperature very low. The minimum on different nights was as low as 12°·5, 17°·5, and 19°. During all that time, though the ground received no heat direct from the sun, and but little in any way from above, yet the supply from beneath was sufficient to keep the temperature of the surface above the dew-point, and the slate and weight in contact with the ground remained black amidst the surrounding whiteness.

Another peculiarity of mornings such as this—which are of frequent occurrence during winter—is the deposition of moisture on trees. During my observations in summer I never saw shrubs dewed to a height of more than a few feet from the ground, while in winter tall trees frequently have vapour deposited on them to the top branches. But as my observations in warm weather are very

limited on this point, and owing to a simple wetting not being so conspicuous as hoar-frost, it is possible trees may occasionally get wet in summer with dew without its being observed. It however seems probable that it will be of much more frequent occurrence in winter than in summer, owing to the much longer absence of the sun during winter nights.

Radiation from Snow.*

In a previous part of this paper reference has been made to the radiating and absorbing powers of snow. In the experiment detailed, comparative readings are given of the temperature of snow and of a black surface exposed in shade on a bright day. The temperatures taken under the conditions then existing showed the snow to be much colder than the black surface. This conclusion has since been confirmed by a number of readings with the radiation thermometer under different conditions of climate. Of these observations it will only be necessary to give those taken on two days. The following temperatures were taken on the 19th January. In these tables the contents of the columns are arranged as in previous one.

Hour.	Air.	Black.	Snow.	Difference.
10.0 A.M.	20°·2	16°·2	12°·0	−4°·2
2.0 P.M.	30°·0	30°·0	26°·0	−4°·0
2.30 „	30°·1	30°·2	26°·4	−3°·8
3.30 „	30°·2	30°·2	28°·0	−2°·2

In the morning the sky was clear, but by 2 P.M. it became overcast, with a thin uniform covering of clouds; and at 3.30, it was beginning to snow. The next readings were taken on the 5th February.

Hour.	Air.	Black.	Snow.	Difference.
10.0 A.M.	23°·0	28°·8	25°·0	−3°·8
12.0 P.M.	25°·5	31°·1	26°·1	−5°·0
2.0 „	30°·0	34°·8	30°·0	−4°·8
3.30 „	31°·0	34°·5	30°·5	−4°·0
5.15 „	31°·5	29°·0	29°·0	−0°·0

On this occasion also the sky was overcast. In these two tables exactly the same result is recorded, the black on both occasions being again much warmer than the snow. These two tables are given, as they were taken under quite different conditions. When the temperatures given in the first table

* Read March 1, 1886.

were taken, the air was warmer than the radiating surfaces, while in the second the air was colder than the exposed surfaces, except towards evening. A number of other readings were taken under different conditions, but with the same results; the snow was always colder than the blacker surface during day, while other observations made at night show their radiating powers are then very similar.

Part of the cooling produced by the snow surface shown in the above tables would be due to evaporation from the snow. The amount due to this cause was not great, as the difference between the wet and dry bulb thermometers was small at the time. I regret, however, that the readings of these instruments have been lost, so I write from memory. It will, however, be observed that the difference in the radiating powers of the two surfaces continued during the whole time the readings were taken on the 19th January, though the weather changed to snow, and the air at that time would be nearly saturated. The readings given in the tables therefore give the total cooling effect of the snow, which is produced by two causes—radiation and evaporation.

This small absorbing power of snow for heat, reflected and radiated from the sky during the day, must have a most important effect on the atmosphere, causing its temperature to be much lower when the ground is covered with snow than when free from it. So that when a country becomes covered with snow—other things being equal—it will be accompanied by a depression of the mean temperature of the air; and, further, as cold tends to produce a stable condition of the atmosphere, not creating the current disturbances of heating, it would appear that once a country has become covered with snow there will be a tendency towards glacial conditions.

But this poor absorbing power of snow is not the only way in which it tends to produce a glacial climate. Snow, in addition to being a bad absorber of the heat of the sky, is also a very poor conductor of heat. In illustration of this, let me mention a few temperature observations made while the ground was covered with snow during January last. On the 18th of that month there was about $5\frac{1}{2}$ inches (140 mm.) of snow on the ground; the night was clear, and radiation strong. At 8 P.M. the temperature of the surface of the snow was 3°, and a minimum thermometer also on the snow showed that it had been at 0° at an earlier hour. Taking a maximum thermometer, the index was brought down below the freezing-point, and the bulb plunged through the snow down to the grass. On examining it a short time afterwards the index was at 32°. In confirmation of this reading, it may be mentioned that on removing the snow the top of the soil was found to be unfrozen. These observations showed that there was a difference of about 30° between the temperature of the top and the bottom of the snow—that is, a distance of $5\frac{1}{2}$ inches.

During the night the temperature of the air fell 5 degrees lower, so that the surface of the snow would be kept about zero for most of the night, yet next morning at 9.45 the bottom of the snow was still at 32°. The temperature of the surface of the snow had risen at this hour only to 8°·5. These observations were repeated on other occasions when the coating of snow was thinner; there was then less reduction in the temperature of the surface, but on all occasions when the snow was a few inches deep, the surface of the soil remained at 32°. As the ground was frozen when the snow fell, it would appear that the earth's heat slowly thawed it under the protection of the snow, and the temperature of 32°, which was below the surface when the soil was frozen, gradually rose to it, where it of course stopped, and the rising heat was spent in melting the snow.

The protection afforded by the bad conducting power of snow is evidenced in our climate, by the amount of vegetation that takes place underneath it, on those occasions when we have had snow on the ground for a length of time. After the snow is gone, many plants are found to have grown, and some advanced nearly to bloom under its protection. This same influence may, however, be seen at work in a more marked manner in spring on the slopes of the Alps and other lands covered with snow all winter. As the snow recedes, and the surface of the earth is gradually laid bare, the vegetation is found to be in an advanced state—many of the flowers, if not in bloom, are just ready to open.

This bad conducting power of snow, compared with soil and rock, will evidently tend to lower the temperature of the air over snow-clad lands, as a few inches of snow in our climate conserves the earth's heat, and prevents its surface being cooled below 32°. The surface of the snow thus gets very much colder than the bare surface of the earth would have been if no snow had been on it, and the air is thus cooled much more over a snow surface than over one of bare earth.

To get evidence of the statement that the surface of a country covered with snow is much colder than it would have been if free from snow, we have only to examine the surface of the ground under the two conditions. The surface of snow covering the ground receives so little heat from the earth that it gets cooled by radiation to such an amount that it is almost always during frost cooled below the dew-point, and is covered with a heavy deposit of hoar-frost. During the late snow I have frequently noticed a thickness of more than 12 mm. of beautiful ice crystals of this kind deposited on its surface. But while the snow is cooled so much, and has this deposit on it, the surface of the soil where it has been laid bare keeps quite free from this deposit, as it receives sufficient heat from below to keep its temperature above the dew-point. The air over snow-clad lands is thus always in contact with a highly-cooled

surface; whereas when there is no snow, the air rests on a warmer one.

Taking then these two things together, the bad absorbing power of snow and its small conducting power for heat, we see that when snow has once got possession of a land, the tendency to glacial conditions is greatly increased; and if it were not for the disturbing effects of heat in warmer climates, it is hard to say how far glacial conditions might spread towards the equator.

Up to the time when the previous part of this paper was given in* I had been unableto find records of any meteorolgical station at which the condition of the ground with regard to snow had been recorded in addition to the usual temperature observations; even yet I have not succeeded in getting suitable records for our climate, and one cannot help regretting that observations of this kind are not more frequently kept in this country.

My conclusions, however, with regard to the effect of snow on climate, I now find are confirmed by the observations of Dr WOEIKOF, an abstract of whose work in this direction has just appeared in *Nature* of 18th February 1886 (vol. xxxiii. p. 379). Dr WOEIKOF has approached the subject from the observational side, and as his results bear directly on ours, I shall quote the following paragraph from the abstract referred to :—

"The year 1877 was a striking instance of how the absence of snow was accompanied by a far less notable lowering of temperature during the prevalence of anticyclones, than would have been the case had the soil been covered with snow. In 1877 there was no snow in Eastern Russia until Christmas, and in November and December the anticyclones occurred, accompanied by no wind, or only by feeble breezes. Quite bright weather lasted in December for more than ten days; and still, in the region which remained uncovered with snow, no great cold was experienced, as usually happens in such circumstances; the minima were 8° to 9° above their average values. The same conditions were noticed during the winters of 1879–80 and 1881–82 in West Europe, as shown by Dr BILLWILLER in the *Zeitschrift für Meteorologie* for 1882."

In the next paragraph, the abstract states that, in the opinion of Dr WOEIKOF, the higher temperature of November, as compared with March in South-East Russia, is due to the ground not being usually covered with snow in November.

These observations of Dr WOEIKOF confirm the conclusions arrived at in this paper from a consideration of the properties of snow. In the opinion of Dr WOEIKOF, the low temperature accompanying the snow is to be attributed to its bad conducting power. While we quite agree with the observer

* Note added 26th February 1886.

that the bad conducting power of snow will be *a* cause of the lower temperature, yet we think that the observations recorded in this paper show that radiation plays a most important part in producing this result. It has been long well known that snow receives less heat from the direct rays of the sun than the surface of a dark body like soil or rock, and we now also know that, under any condition of sky yet tested, it absorbs but little of the sky radiation. From this we see that as the surface of the snow, under all conditions of sky, receives less radiant heat from without than the surface of the ground, the air in contact with it will be more cooled than the air over bare soil or rock.

In the radiation temperatures above recorded, the snow was generally about 4 degrees colder than the black surface during the day. We must not from this suppose that these 4 degrees are the greatest difference that could exist in the radiation temperatures of the two surfaces. These 4 degrees simply represent the difference that was maintained under the conditions when the tests were made. If the air circulation had been greater, the difference would have been less, and both would have been nearer the temperature of the air; and if the circulation had been less, the difference would have been greater. But while there may be a variation in the difference of the temperatures of the two surfaces, there will be but little difference in their respective cooling effects, as with a quick circulation a large amount of air will be cooled a little, while, when the circulation is slow, a little air will be cooled a great deal. Not only so, but on account of the temperature of space being so much colderthan the surface of the snow, the temperature of the snow will tend to sink about the same amount below the temperature of the air, even though the air be greatly cooled; so that, under the same conditions of radiation and atmospheric circulation, however much the air may have been cooled, the two surfaces will tend to maintain the same difference in temperature, and the snow will always tend to cool the air more than a black surface, when the surfaces are colder than the air, or to heat it less when the surfaces are warmer.

It is therefore evident that this radiation effect will be one of the causes tending to produce the lower temperature of the air while snow is on the ground, and it seems probable that the snow will not only reduce the minima temperatures, but also the maxima. It seems also probable that the bad absorbing power of snow will have a most important influence in retarding the approach of warm weather, as the earth under snow receives far less heat from the sun with the approach of spring than if the ground had been free from snow. This is, of course, altogether apart from the question of the retardation of the approach of warm weather by the sun's heat being spent in melting the snow, instead of warming the air.

Summary.

Our principal conclusions may be summed up as follows:—*First,* From the experiments made with (a) the inverted trays, (b) by weighing small areas of turf, and (c) by observations of the temperatures on and under the grass during dewy nights, we have concluded that vapour is almost constantly rising from grass land, by night as well as day, in our climate. *Second,* From experiments made with (a) inverted trays, (b) by weighings of small areas of soil, and (c) by observations made with small test condensing surfaces, on dewy nights, we have concluded that, under most conditions of our climate, vapour rises from uncultivated areas of soil during night as well as day. It follows from these two conclusions that dew never "falls" on the earth; and for reasons given, it is only deposited on plants, and other bodies, not in good heat communication with the ground. *Third,* That the greater part of the dew condensed on bodies near the ground is formed of the vapour rising at the time from the earth, and very little of it from the vapour that rose during the day. *Fourth,* That dew forms copiously on roads, but owing to the stones being good conductors of heat, the vapour is deposited on the under sides of the stones, and not on the top as on grass. *Fifth,* Wind hinders the formation of dew by preventing an accumulation of damp air near the ground. *Sixth,* The "dew-drop" formed on grass and other plants is not dew at all, but is formed of the exuded sap of the plant. *Seventh,* Almost all substances, such as black and white cloths, garden mould and grass, radiate equally well at night. Among the few exceptions observed are polished metals and sulphur. *Eighth,* A covering of snow on the ground lowers the mean temperature of the air.

FURTHER REMARKS ON DEW.

Appendix.

(Read 19th July 1886.)

Since the preceding paper was written, a few opportunities have occurred for continuing the investigation under different conditions of climate, and some additions have also been made to strengthen the exudation theory of the "dew-drop" on grass and other plants, of which I shall here give a short account.

At the beginning of the paper, evidence is adduced to show that water vapour is almost constantly rising from the ground during night, as well as day, and it is there noted that the experiments were somewhat unsatisfactory on account of the rather damp condition of the soil at the time, the dry season being over before the investigation was made. In order to supplement these observations, a few others were made in the beginning of July of this year, when the soil was in about as dry a condition as it almost ever is in this climate.

An inverted tray was placed over bare soil which was so dry and powdery that it rose in dust when the edge of the tray was pressed into it. The tray was not placed on the ground till after 9 P.M.—that is, after the soil had lost a good deal of heat and moisture—yet in two hours the inside was wet, and in the morning it was covered with drops. While this experiment was going on, another tray was placed on the lawn at a place where the grass was burnt quite brown. The inside of this tray was also wetted, and to about the same amount as the one over the bare soil. It may be noted here, that the positions for making these tests were selected on account of their extreme dryness. The bare soil was light and open, while the lawn was extremely dry, having been laid down a few years ago with every precaution to ensure this condition; the under soil is dry and sandy, it was moreover well drained, the upper soil was removed, and a good depth of ashes put in its place, then a layer of sand, on which was put the turf with its inch or two of soil. A drier position could scarcely have been selected for the trial, and yet the result showed that in our climate bare soil and grass land, even when very dry, continue to give off vapour during dewy nights.

A few experiments were also made on this subject in March last at Hyères, in the south of France. In order that the test might be as severe as possible, I selected a spot where the soil seemed driest and most exposed to the sun and the wind; and as the soil was stony and lying on the hill-side, it could have no supply of water from below. An inverted tray placed over this arid ground collected a surprisingly great amount of moisture. The following are the notes of one of the tests made at Hyères on the 27th March last:—At 5.15 P.M. the sun had ceased to shine on the place selected for the experiment; at that hour the temperature of the soil was 73° F. at 3 inches below the surface, and 59° at 12 inches. At 5.45 the tray was put on the ground. Examined at 7.15 P.M. the outside was dry, but inside was quite wet, and by 10 P.M. the drops were so large they ran down the inside when the tray was placed vertically. This great amount of wetting was probably due, not only to the ground being highly heated during the day, but also to a considerable fall in temperature at night, by the cooling produced under the clear skies of that climate.

I much regret I have been unable to get any trustworthy information regarding the movement of the vapour near the surface of the ground in barren and desert countries. I have, however, received some information from travellers who have been in Australia and parts of South Africa, where rain does not fall for months at a time, and it goes to prove that even in these dry countries vapour rises from the ground at night, as they often found the under side of their waterproof bedding placed on the ground to be wet after camping out at night. One would scarcely have expected much moisture to be collected in this way on account of the warmth of the sleeper's body keeping up

the temperature of the condensing surface. It therefore seems probable that the moisture will collect under those parts of the waterproof which are beyond the influence of the sleeper's body.

While the experiments above referred to on bare soil and grass land were being made on ground exceptionally dry in July, those with slates placed on the road were repeated. As in the experiments described in the first part of this paper, the slates placed on the hard dry part of the road and on the gravel got quite wet on their under sides at night, thus showing that, even under the exceptionally dry conditions existing at the time, vapour was still rising at night from the hard and arid roads.

In connection with the action of stones lying near the surface of the ground, we may here refer to a result observed by agriculturists, on which it seems to throw some light. It has been remarked that the removal of small stones from fields where the soil is light and open, often has a prejudicial effect on the crops. It must be admitted that accurate information on this point is not easily obtained, but the impression in some parts of the country certainly is, that the removal of small stones from fields is not to be recommended. Now the removal of stones may act prejudicially in different ways. In some cases the disintegration of the stones may add to the richness of the soil; their removal may therefore in some cases decrease the natural fertility of the field. No doubt the removal of the stones will permit of a more rapid evaporation from the soil during the day, but it does also seem probable that the peculiar action of stones, in trapping the moisture before it comes to the surface at night, will have a beneficial effect on light and dry soils. Part of the moisture is trapped by them before it can escape from the surface at night; and when the sun rises, the stones becoming warmer than the soil under them, the moisture leaves the stones and condenses in the soil underneath. In this way stones would seem to have a sort of conserving action on the moisture, and tend to check the prejudicial effects of continued dry weather.

In confirmation of this conserving action of bodies lying on the surface of the ground and improving its fertility, I would refer to a letter in *Nature*, vol. xxxiii. p. 583, by Lieut.-Col. A. T. FRASER. As this letter is so interesting, and bears directly on our subject, I may be allowed to quote from it here. At the beginning of his letter he says :—"Having had occasion to lay out a large quantity of iron hoes and picks, without handles, on the hard ground of an open inclosure in one of the driest districts of India (Bellary), where, in fact, these implements had been collected in the face of a scarcity, it was found, after they had lain a couple of months, that a thick, weedy, but luxuriant vegetation had sprung up, enough, though there was no rain, to almost hide the tools."

Lieut.-Col. FRASER also says he had previously noticed in the tropics a

similar stimulating effect produced on vegetation when tools were left lying on the surface, but was unable to account for it till he read the abstract of the first part of this paper, when he at once recognised the manner in which the tools lying on the surface acted in hot and dry climates by checking the escape of vapour at night. No doubt much of the increased fertility observed would be due to the wetting the plants received by the vapour condensed on the under sides of the tools; still it will be admitted something must also be owing to the reduced evaporation during the day, produced by the metal tools checking the escape of the vapour.

This conservation of moisture by stones and other bodies lying on the surface of the ground, and its up and down movement during night and day, may be easily seen in the experiment with the slates. Examined at night the *road under the slates is as dry* as the exposed parts, while the under sides of the slates are wet; but if examined at a certain time in the morning, the under sides of the slates will be found to be dry, *while the road under them is wet.* But, if the slates are not examined till a later hour, both the slates and the road under them will be found to be dry, the moisture being driven deeper and condensed among the stones lower down.

Many people, after reading the conclusions arrived at in the first part of this paper, seem to have a difficulty in accepting the theory that vapour is constantly rising from the ground during night as well as day, and that the dew on grass is formed of this rising vapour, because it seems to them to contradict the teaching of Dr Wells, and it also appears to be at variance with their experience. A little consideration will show that the results established by Dr Wells are in no way affected by it. That investigator certainly did not think that much vapour rose from the ground at night, yet he was well aware that some might rise; his investigation was however principally confined to the condensation of the vapour after it is in the air, and he gave comparatively little attention to its source; whereas in this investigation Wells' results are accepted with regard to the condensation, and an attempt is made to extend the subject by investigating some parts not worked out by him.

Others again have made a difficulty to the acceptance of this theory by extending the conclusions arrived at in this paper to other conditions than those for which they are true. They have assumed that if dew on grass and on bodies near the ground is formed of vapour rising at the time, then the dew found on bodies higher up in the air must also be formed of vapour rising at the time from the ground immediately underneath; and as this conclusion is opposed to experience, they seem inclined to dismiss the whole theory as unworthy of consideration. When we come to investigate what is taking place in nature we will see that this extension of the conclusion is by no means justified.

The reason why it is concluded that the dew on the grass, and on bodies

close to the ground, is formed of vapour rising from the ground, is that at night the ground under the grass is always in a condition to give off vapour, and this rising vapour will tend to displace that which rose during the day; the stems and blades of grass will thus become surrounded by vapour that has risen during the night. A further reason is, that the ground under the grass is much warmer than the air over it, and the air in contact with the moist earth is nearly saturated; the tension of the vapour in the air rising through the stems of grass is thus higher than that in the air over them, and it is therefore in a more favourable condition for condensing and forming dew than the air higher up. This hot rising vapour will often indeed yield dew when the air over the grass can give none.

When, however, we come to consider what takes place higher up above the grass, or even at the tops of the blades, we meet with a much more complicated condition of matters, and we are now able to say very little about the source of the vapour condensed at these higher positions. Whenever we get above the protection of the grass, into the parts of the atmosphere exposed to air-currents, we can say very little as to the source of the vapour existing there, either as to the place where it changed to vapour, or the time when this change took place. No doubt some of the molecules in this upper air will have risen but recently from the ground, but some of them will certainly—if there is the slightest wind—be molecules that have risen during the day, and no doubt some of them will have ascended into the air many days previously; and while some will have but recently come from the ground immediately underneath, others will have travelled from lands and oceans far away. But while this may be so, it in no way affects the conclusion that vapour is almost constantly—night as well as day—given off by the ground, and that dew on grass, and on bodies close to the ground, is part of this rising vapour trapped by their cold surfaces.

Curiously enough, history here repeats itself. The theory that dew rises from the ground has before now been wrecked by the observation that dew forms on bodies placed high above the ground, and in situations where no vapour could have risen to them from beneath. Professor MUSSCHENBROEK rejected GERSTEN'S theory of rising dew after he found dew was deposited on bodies placed on the leaden roof of his observatory. He thought the dew formed under those conditions could not have risen from the ground, but must have fallen from the atmosphere. There must, therefore, evidently be a foundation somewhere for the rejection of the theory on the grounds stated, though it must be admitted it is difficult to find, as the statements are in no way opposed to each other. It will, therefore, be as well for us to consider here the cause of this appearance of opposition.

One cannot help thinking that a good deal of the difficulty experienced in

reconciling the statement, that the dew formed on bodies near the surface of the earth is formed of vapour rising from the ground, with the fact that dew is found on bodies high up in the air is caused by a want of clearness in our ideas, or perhaps rather to a persistence of primitive ideas. Dew was in olden times often spoken of as falling from the heavens, and even yet we talk of falling dew. This expression is to a certain extent associated with the idea of falling rain—a process in which the moisture passes from place to place through the air, and falls on bodies exposed to it; and many seem to think that if dew comes out of the ground it should be found only on bodies exposed to the earth. That in fact rising dew is the converse of the old falling dew, whereas dew is only so much moisture taken by a cold surface from the store of vapour in the air. This explanation of the difficulty does seem somewhat absurd, but we all know that old habits of thought have a curious way of asserting themselves, and it seems the only way of explaining a difficulty so many have felt.

Let us picture an imaginary state of matters, in which all the conditions shall be as simple as possible. Suppose the vapour to be constantly rising from the ground, and that the air is absolutely still; and further, let us imagine that the vapour flows upwards through the air in a continuous stream, only varying in velocity at different hours. At 6 P.M. we will suppose the molecules of vapour that left the earth at 6 A.M. to have arrived at a height a. Let dew now begin to form, then the moisture condensed at 6 P.M., on bodies at the height a, will be vapour that rose from the ground at 6 A.M., and bodies at intermediate heights will have the vapour on them that rose at the intermediate hours. At 6 A.M. the following morning the vapour that rose at 6 on the previous evening will be at an elevation b, and the dew forming on all bodies lower than b at this hour will be vapour that rose during the night. So that in these ideal conditions, if we knew the rate of ascent of the vapour, we could tell the hour at which the vapour—condensed at any height—rose from the ground.

If this imaginary condition of matters was correct, then there would be some reason for expecting that dew would only be deposited on bodies placed over such areas as yield vapour. In nature, however, the conditions are much more complicated. The vapour does not flow upwards in a uniform stream, but is mixed with the air by eddies and wind currents, and carried to bodies far from where it rose; so that while we can say something about the source of the vapour condensed on bodies near the surface of the earth, yet the molecules in the higher air have no history we can interpret. While the vapour rising from the ground plays an important part in the phenomena of dew, as it is not only the source of that formed on bodies near the ground, but it also increases the amount deposited on bodies high up, yet the rising vapour is not essential to its formation, as dew may be deposited even though the country for many miles all round is dry and incapable of yielding any vapour. In such case

the supply of vapour to form dew would depend on the evaporation of the dew, and on what was brought in by the winds.

The "Dew-Drop."

The statement that the "dew-drops" formed on plants at night is not dew at all, but is formed of the exuded sap of the plant, has been rejected by some on account of its being contrary to all accepted ideas on the subject, while some who have accepted it, have given only an indifferent assent. It has therefore seemed desirable that further evidence be advanced in support of the statement, and also that some simpler methods be devised for studying the phenomena connected with the exudation of moisture by plants, so that those not accustomed to making difficult experiments may be able to demonstrate the point for themselves.

One of the simplest experiments of this kind is to cut a piece of turf, or, better still, lift a single grass plant with a clod of earth attached to it, which can generally be easily found in any garden. The leaves and stems of a single plant being separate and open, the phenomena are more easily observed on it than in the confused vegetation of a turf. Place the plant on a plate, and invert a tumbler or other vessel over it, so as to enclose the plant and rest it on the plate. This should be done when the soil is not too dry, otherwise water will require to be given to the plant. After it has been kept in moist air for about an hour drops will begin to exude, and the tip of nearly every blade will be found to be studded with a diamond-like drop.

In the above simple experiment there is nothing to tell us where the moisture came from to form the drops. It might be contended that it was condensed by the plant out of the moist air. It has, however, been shown in the first part of this paper, that when the tip of the blade is isolated from all supply of moist air, the drop at the end grows as quickly as the drops at the ends of the blades exposed to saturated air. This experiment is, however, a somewhat difficult one for any one not accustomed to work of this kind. The point may, however, be proved in a much simpler way. Take any exuding plant with a single stem, such as a broccoli or poppy, if it is growing in a pot so much the better, as it is more convenient both for making and seeing the results of the experiment. Prepare a circular disc of metal—say tin-plate—with a hole in its centre large enough for the stem of the plant to pass through, then cut the disc in two through the centre. Now place the disc on the pot with the stem of the plant passing through the hole, and join the two halves of the disc, either by soldering, or by cementing over the joint a strip of sheet india-rubber. A large glass receiver is now placed over the plant with its edges resting on the metal plate. In this way the plant is isolated in air from which it can extract no moisture, the metal plate preventing vapour

rising from the soil underneath. If any drops now appear on the leaves, it is evident the air cannot be the source of the moisture.

When tested in this way, it will be found that any exuding plant will soon become studded with drops, and present exactly the appearance it would at night. The time the drops take to appear depends on certain conditions; amongst these are—1st, the kind of plant experimented on; 2nd, the state of vital activity in the plant at the time; and 3rd, the degree of dryness of the leaves at the time it is put under the receiver. If the leaves are very dry, it will take some time to fill the tissues of the plant with sap before the surplus begins to exude. A broccoli plant in fair health and condition may be expected to show drops in less than an hour. These drops are small at first, and gradually grow so large that they fall off by their own weight.

An experiment of this kind is so easily made by any one, that the interest and the information gained is ample reward for the little trouble taken in making it. Instead of using a metal plate as above described for isolating the plant from the damp soil, a simpler plan is to use a piece of sheet india-rubber, with a radial slit in it, for slipping it round the plant, the two edges being joined with india-rubber solution, the glass receiver being, as before, placed over the plant, and its edges resting on the rubber.

The evidence advanced in the first part of this paper in support of the statement that the "dew-drop" is exuded by the plant is—1st, that the drop at the tip of a blade of grass grows as quickly when isolated from all supply of moist air as when exposed to saturated air; 2nd, the blades of those plants which have drops attached to them on dewy nights exhibit no drops when separated from the root, even when supplied with water and placed in saturated air; and 3rd, when hydrostatic pressure is applied to the stalk of the leaf of any plant that has drops attached to it at night, it causes the leaf to exude at exactly the same points as the drops appeared on dewy nights. Although the above evidence is fairly conclusive, yet there is a point where it might be strengthened by the addition of a link to the chain. While it is shown that hydrostatic pressure will produce the same effects as are seen on plants at nights, yet no evidence is adduced to show that there is any internal pressure in those plants on which "dew-drops" have been observed. This omission has now been corrected, and some experiments made in connection with this point. The plants selected for experiment were of the same kinds as those which were observed to have "dew-drops" on them at night, and which showed exuded drops when subjected to hydrostatic pressure, such as broccoli, cauliflower, poppy, &c. For convenience, some plants were grown in flower pots, and the experiments made while they were still small.

For measuring the pressure inside the plants, U-shaped tubes half filled with mercury were used, and the pressure measured by the height to which

the sap forced the mercury. These gauges did their work well enough, but they were somewhat slow in action, as it takes some time for sufficient liquid to be exuded to displace the mercury and force it up the tube. Where high pressures were required to be measured, the U tubes had to be abandoned, on account of their inconvenient height, as well as the length of time required for making an observation with them, and the pressures were measured by means of air-pressure gauges. These gauges were made of a short length of wide thermometer tube, having a bore of less than 1 mm. diameter, a short column of mercury being put in to form an index. The pressures were calculated from the volumes, and corrections made when necessary for temperature. The gauges were occasionally compared with a column of mercury to see that everything was correct. The pressures given cannot be considered correct to more than 10 mm. of mercury, but for the present purpose this degree of accuracy is sufficient, as the pressures are very indefinite, varying with so many conditions that anything like an exact figure cannot be looked for in experiments of this kind.

I shall now describe in detail an experiment made on a cauliflower, as it is similar to those made on other plants, of which it will only be necessary here to give the results. The pot containing the plant was placed on a sheet of metal, and a glass receiver got ready large enough to cover it entirely. The stalk of one of the blades was selected for making the connection between the pressure gauge and the plant. This stalk, while the leaf was still on it, was prepared for making a water-tight joint by filling up the longitudinal groove in its upper surface with beeswax, laid on with a slightly heated iron. The blade was now cut off, and the gauge attached by means of a short length of soft india-rubber tube; the stalk having been made round by means of the beeswax, a tight joint was easily made. With the exception of this one leaf cut off, all the others were left untouched. The receiver was now put over the plant to stop evaporation from the leaves, and everything left at rest. After a short time the pressure was seen beginning to rise in the gauge, and drops also began to show themselves all round the edges of the leaves. As time went on the drops increased in size, and the pressure went up to 290 mm. of mercury, at which point it stopped. This pressure can be considered correct only for the particular plant under the particular conditions. It simply meant that when the pressure rose to 290 mm., the whole of the supply of sap sent up by the root could find an exit by exudation. If the supply had been greater, or the exuding pores fewer or smaller, the pressure would no doubt have gone higher, and *vice versa*.

The plant in the above experiment was in the same condition as it would be on a dewy night. All evaporation from the leaves was stopped, and transpiration having ceased, the root continued to send up its supplies of sap, first filling

the tissues of the plant, and then producing an internal pressure, which forced the sap to escape by the exuding pores. But what is the condition of the tissues during day when transpiration is going on? An answer to this was easily obtained by removing the receiver from the plant, and allowing evaporation to proceed from its leaves. The result was that the pressure inside the plant fell, and a negative pressure took its place; the mercury first fell in the U tube, and then rose on the other side. The mercury was drawn up in one case to a height of 140 mm., and in another plant to 180 mm., the height seeming to depend on the rate of evaporation, and the perfection and closeness of the tissues of the plant enabling it to stand a greater or less pressure before air forced its way inwards. We see from this that exuding plants during night, and at times when there is little evaporation, have an internal pressure tending to distend their tissues, and have a negative or external pressure during the day tending to press the tissues inwards. This internal pressure may help to explain the more rigid appearance of the leaves of plants at night; while the negative pressure or degree of vacuum produced inside the leaves by transpiration explains the manner in which water is taken up by cut flowers and branches of plants when their ends are placed in it, and it also explains something of the peculiar curving of leaves when withering.

I have said that the pressure above measured inside of the cauliflower plant would have been much greater if the exuding pores had been less in size or number—that, in fact, the pressure then measured was not the maximum root pressure. To test this point, the plant was now cut across near the bottom of the stem, within two or three centimetres of the root, so removing all the leaves with their exuding pores; and the pressure gauge was attached to the stem near the root. The gauge now rose very rapidly, and in a short time indicated a maximum pressure of 760 mm., the india-rubber connecting tube requiring to be strongly bandaged to prevent it bulging. It seems strange that the delicate tissues of a young plant should be able to produce and resist so great a pressure. We must however remember that this last registered pressure is one to which the plant is never subjected when under natural conditions, but even the 290 mm. measured when the plant was exuding freely does seem a great pressure to exist in plants.

A poppy tested in the same way showed an internal pressure of 175 mm. with its leaves all on, and exuding freely in saturated air. This lower pressure compared to the cauliflower, would seem to indicate that the exuding pores are larger or more numerous in the poppy than in the cauliflower, as the former are fully as wet as the latter on dewy nights. When the poppy was cut across, and the gauge attached to the main stem near the root, the pressure rose to as much as 1040 mm.

The pressure inside grass has not been easily measured, owing to the diffi-

culty of making a tight joint with the gauge, and so delicate a structure as a grass stem. The highest pressure observed before the joint gave way was 160 mm. The measurement was taken with all the blades on, and exuding freely in saturated air. This pressure is just a little less than was found in the poppy when under similar conditions. No measurements have been made with all exudation stopped, on account of grass not growing in a form suitable for making a measurement of this kind.

The following are a few readings of the maximum root pressure given by different plants. These plants were small and still in the seed-bed, but too large for transplanting.

Cauliflower, ...	875 mm.
,, ...	920 ,,
,, ...	1065 ,,
Cabbage, ...	1310 ,,

From the above figures it will be seen that the pressures given by the different cauliflower plants varied considerably; and that the cabbage experimented on was capable of exerting a root pressure equal to forcing its sap to a height of about 58 feet, thus showing an extraordinary reserve of energy.

It was shown in the first part of this paper that the leaves of these plants exuded when hydrostatic pressure was applied to their stalks, and we have now shown that there is abundance of pressure inside these plants at night to produce the exudation. Nothing like an attempt, however, has been made here to give either the exact pressure inside different kinds of plants or the pressure under different conditions. It has already been stated that the conditions affecting the pressure are much too varied for the figures to be settled by a few experiments; all that has been attempted is to show that in exuding plants, there is abundance of pressure to produce the results claimed.

In connection with this subject, it was interesting to notice the variation in the exudation of grass during the late continuance of dry weather. While the soil was damp exudation went on as usual, but when the ground got drier the exudation gradually got less and less, and at last it entirely ceased. Even when the grass was covered with an enclosure, and surrounded with saturated air, no exudation took place, and yet the grass was green and growing; it took some time after the grass was wetted before the activity was great enough to give rise to exudation. During this dry weather, while the grass had ceased to exude, it got moist at nights with the hot vapour rising from the ground, the lower parts, particularly where exposed to radiation, being wetter than the tops of the blades.

It is very difficult to get an idea of the number of plants that exude, so much depends on the vitality of the plant at the time, and on the amount of

moisture in the ground, so that the same plant may emit drops at one time and not at another. We have seen that even so free an exuding plant as grass may cease to discharge; others cease with a less degree of dryness, and with a less decrease in vital activity. The number that exudes under favourable conditions is, however, much greater than we might at first imagine. In the south of France, in spring, a very great number of plants were observed to exude on dewy nights; even roses had their leaves fringed with drops, a condition in which I have never seen these plants in this country; but the activity of vegetable life in spring is very much greater in the south than with us.

This question of root pressure in plants is one of vast interest; so much still remains to be known about it. How is it that one plant must have the soil in which it grows full of water, while another requires it to be only damp? Another seems to be able to grow on nearly dry soil, whilst another still can by means of its air-roots extract moisture from air that is not saturated. What is the source of energy called into action by this latter class to enable it to condense the vapour in the air? Is it a chemical process? or a purely physical one, like the condensation of vapour by Professor Tait's hygrometer when it is falling? or is it some unknown function of vitality? These questions, however, open up a field much too wide to be considered here.

28*th July* 1886.—After I had written the above paragraph, and as I supposed had closed the paper, it slowly dawned upon me that the surface of the leaves of all the different kinds of plants that have been observed to exude drops behaved themselves in a particular manner towards water. None of them seemed to be wetted by it. The glistening rain-drop on the grass shows that the blades of that plant are not wetted by water, the glistening being due to the reflection from the inside of the drop, where it rests on the blade, but does not touch it. But do all the other exuding plants repel water in the same manner? As it was raining while these thoughts passed through my mind, a visit to the garden was at once made, and the broccoli, poppy, and all the other exuding plants were examined. Every one of them was found to behave towards the rain-drops in the same manner as the grass. The rain-drops slipped off their surfaces "like water off a duck's back;" and where water collected in the hollows of the blades, the reflection from its internal surface showed it was not in contact with them.

The other plants—cultivated and uncultivated—in the garden were then examined, when most of them were found to be quite wet. The difference in their appearance from the exuding ones was very marked. At first sight the leaves of plants that got wet, like potatoes, beans, &c., looked almost as if they were dry, but in reality the water wetted them so perfectly all over, that it ran off, leaving only a thin and even film on their surfaces; whereas all the plants that exuded drops had their surfaces dry, save certain small areas

where the natural surface of the blade had been destroyed. On thinking over the matter, it became evident that this property of leaves that exude drops at night ought to have been foreseen by me. The fact that the emitted moisture remains as a drop, shows that the surface of the leaf rejects water; if the leaf surface got wetted with water, the exuded liquid would have crept outwards from the exuding pore, and have wetted the leaf for some distance all around it. These exuded drops behave very much in the same manner as a drop of water attached to the end of a glass rod that is not very clean; the water does not wet the rod, but draws itself up into a drop. If the drop had been attached to a wooden rod or a piece of thread, or anything that was easily wetted, it would not have remained as a drop, but have spread itself all over the surface of the body.

When examining the plants in the garden during rain, in addition to those plants which I knew exuded drops at night, I noticed a number of others that rejected the rain drops, and kept their surfaces dry in the same manner as the exuding plants. Amongst these were Nasturtium, some of the Brassiceæ family not previously observed, and also some weeds. Now, it appeared that if the above reasoning is correct, these other dry-surfaced plants ought to exude drops, I therefore marked them, and on afterwards experimenting found that they also discharged drops like the others.

It almost looked at first sight as if this property of repelling water was a distinguishing characteristic of the leaves of all exuding plants; but on further considering the matter, the idea soon suggested itself that the other class of plants, the leaves of which got wetted with rain, might also exude moisture, as it was evident that if they did exude the discharge would be masked, for the moisture would not collect on them in drops, but spread itself over the leaves, and so become undistinguishable from dew. It therefore seemed desirable that other experiments be made on this class of plants, to see if any of them exuded moisture. It was evident that special precautions would be necessary to enable us to see the exuded moisture on leaves easily wetted, as it would not be so easily seen as the sparkling drop on water-repelling leaves.

For investigating this point, the most convenient plant I could find was a strong growing variety of everlasting flower (Helichrysum). This plant was one of those observed to have its leaves wet while it was raining, and no exuded drops were observed on it at night. The first thing determined was to see if there was any root pressure to cause exudation. The plant was cut across at the bottom of the stem, and the pressure gauge attached near the root. The root pressure was found to be 950 mm.; that is, this plant had as great an internal pressure as was found in the drop-exuding plants. In order to see whether it exuded when hydrostatic pressure was applied, the upper part of the plant, which was cut off for taking the root pressure, was removed to the

laboratory, where it was connected by means of an india-rubber tube with a head of water of about 1·5 metres, and surrounded with saturated air. After a time drops appeared at the tips of most of the leaves, and also at some other points on them; but these drops were quite unlike those on grass, broccoli, and other water-repelling plants; they spread themselves on the leaves, and adhered to them, no reflection being given from the back of the flattened drop. It could, however, be easily seen, when the experiment was made in this way, that moisture is exuded from the plant, whereas at night no exuded moisture is perceptible. The reason for this is, that under the condition of the experiment, the exuded drop only spreads to a certain extent, and the outline of the wetted surface is defined, because the whole surface of the leaf is not wet; but at night the surface of the leaf is wet with dew, and the exuded drop spreads and thins away by imperceptible degrees into the dewed surface. This was illustrated in the above experiment by breathing on the leaf, so as to bring it into the same condition it is on dewy nights, the drop was then seen to spread rapidly outwards.

We see from the above that a plant may be exuding, and yet we may not be able to notice it. This is specially the case while dew is forming, that is under natural conditions; for dew is very generally forming while plants are exuding, and it is difficult to tell from an examination made at night whether any plant whose leaves have an affinity for water is exuding or not. It is therefore much better to test the plants under artificial conditions, by placing them in saturated air, but where no dew can be formed on their surfaces. This can be done by placing them at night under hand-glasses, and well protected from radiation, or even during the day under metal boxes, and well shaded. In this way a few plants, whose leaves got wet with rain, were tested, and all were found to exude if the evaporation from the leaves was stopped long enough, and time given for the tissues to get filled with sap. In all cases the exuded moisture adhered to the leaf and formed a wet patch. The plants tested were helichrysum, stocks, asters, mignonette, foxglove, celery, lettuce, turnips.

The plants were taken at hazard, and while some, such as mignonette and stocks, exuded little, the others discharged a good deal. The root pressure of a stock was measured, and found to be only about one-half that of the more freely exuding Helichrysum. The *root pressure* will, however, be only one factor in determining the amount exuded, as it is evident the *rate of supply* sent in by the root will be of as much importance; but no measurements of quantity have been made by me. It may be as well to note here, that though the few plants, taken at hazard, all showed powers of exuding, yet we must not therefore conclude that all plants have this property.

It is interesting to note the effects of these two ways in which the surface of leaves behave towards their exuded sap and water. Take the different kinds

of turnips, for instance. The Swedish variety exudes freely, the liquid forming little drops fringing the leaves, while the moisture exuded by the other varieties spreads itself over the leaves. One result of this is, that after dewy nights the softer varieties dry sooner than the Swedish, because the exuded moisture, by spreading itself over the surface of the leaves, dries up much more quickly than the drops on the others. This seems to be the explanation of a fact frequently observed by sportsmen and others who have occasion to walk through turnip fields on autumn mornings, namely, that the softer varieties generally wet them much less than the swedes. Again, after rain the swedes take longer to dry than the others, because their surfaces do not get wet, but the water collects in drops, imperfectly attached to them, and also fills the hollows of their leaves; whereas the other kinds get wet, and the water runs off them, leaving only a thin film on their surface, which dries up much more quickly than the drops on the others. Further, when we walk through turnips immediately after rain, our feet brush the drops from the swedes in showers, which rapidly wet us, while the water adheres to and does not so easily leave the surfaces of the others.

This last part of the investigation takes us a step further, and shows us that not only is the dew-drop a result of the vitality of those plants on which it forms, but that much of the wetness spread over the leaves of others on dewy nights is produced by moisture exuded by the plants.